PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIII. 1884-85. No. 119.

THE 102ND SESSION.

GENERAL STATUTORY MEETING.

Monday, 24th November 1884.

The following Council were elected:—

President.

THOMAS STEVENSON, M. INST. C.E.

Vice-Presidents.

Rev. W. LINDSAY ALEXANDER, D.D.
ROBERT GRAY, Esq.
A. FORBES IRVINE, Esq. of Drum.
EDWARD SANG, LL.D.
DAVID MILNE HOME, Esq. of Milne-Graden.
JOHN MURRAY, Esq.

General Secretary—Professor TAIT.

Secretaries to Ordinary Meetings.

Professor TURNER, F.R.S.
Professor CRUM BROWN, F.R.S.

Treasurer.—ADAM GILLIES SMITH, Esq., C.A.

Curator of Library and Museum—ALEXANDER BUCHAN, Esq., M.A.

Ordinary Members of Council.

Professor COSSAR EWART.
Professor JAMES GEIKIE, F.R.S.
Rev. Dr W. ROBERTSON SMITH.
STAIR AGNEW, Esq., Registrar-Gen.
Prof. DOUGLAS MACLAGAN, M.D.
The Hon. Lord MACLAREN.
Rev. Professor FLINT, D.D.
Professor T. R. FRASER, M.D.
Professor CHIENE.
J. Y. BUCHANAN, Esq.
Professor CHRYSTAL.
Professor DICKSON.

By a Resolution of the Society (19th January 1880) the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

HIS GRACE THE DUKE OF ARGYLL, K.T., D.C.L.
SIR WILLIAM THOMSON, LL.D., D.C.L., F.R.S., Foreign Associate of Institute of France.
THE RIGHT HON. LORD MONCREIFF of Tulliebole, LL.D.

BRITISH MUSEUM

Monday, 1st December 1884.

THOMAS STEVENSON, Esq., Memb. Inst. C.E., President,
in the Chair.

The President opened the Session with a short Address, and an Obituary Notice of Sir Alexander Grant.

The following Communications were read:—

1. On the Distribution of Energy between colliding Groups of Molecules. By Sir W. Thomson.

2. On the Dynamics of Reflection and Refraction in the Wave Theory of Light. By the Same.

3. On Kerr's Discovery regarding the Reflection of Light from a Magnetic Pole. By the Same.

4. On an Improved Method of Measuring Compressibility.

By Professor Tait.

When the compressibility of a liquid or gas is measured at very high pressures, the compression vessel has to be enclosed in a strong cylinder of metal, and thus it must be made, in some way, self-registering. I first used indices, prevented from slipping by means of hairs. Sir W. Thomson's devices for sounding, at small depths, by the compression of air, in which he used various physical and chemical processes for recording purposes, led me to devise and employ a thin silver film which was washed off by a column of mercury. Much of my work connected with the *Challenger Thermometers* was done by the help of this process. Till quite recently I was unaware that it had been devised and employed by Cailletet in 1873, only that his films were of gold.

But the use of all these methods is very laborious, for the whole apparatus has to be opened *for each individual reading.* Hence it struck me that, instead of measuring the compression produced by a given pressure, we should try to measure the pressure required to produce an assigned compression. I saw that this could be at once effected by the simplest electric methods; *provided that glass, into which a fine platinum wire is fused, were capable of resisting very high pressures without cracking or leaking at the junctions.* This, on trial, was found to be the case.

We have, therefore, only to fuse a number of platinum wires, at intervals, into the compression tube, and very carefully calibrate it with a column of mercury which is brought just into contact with each of the wires successively. Then if thin wires, each resisting say about an ohm, be interposed between the pairs of successive platinum wires, we have a series whose resistance is diminished by one ohm each time the mercury, forced in by the the pump, comes in contact with another of the wires. Connect the mercury with one pole of a cell, the highest of the platinum wires with the other, leading the wires out between two stout leather washers; interpose a galvanometer in the circuit, and the arrangement is complete. The observer himself works the pump, keeping an eye on the pressure gauge, and on the spot of light reflected by the mirror of the galvanometer. The moment he sees a change of deflection he reads the gauge. It is convenient that the external apparatus should be made to leak slightly; for thus a *series* of measures may be made, in a minute or two, for the contact with each of the platinum wires. Then we pass to the next in succession.

I have found this method perfectly successful in practice, enabling me to do in an afternoon (and far more certainly than before) as much as I formerly could manage in a month. But I cannot properly apply it to the compression of water at various temperatures (from 0° to 100° C.) until I get a new, light but strong, steel compression apparatus, which has been ordered for this special problem.

The experiments hitherto made by this process (to satisfy myself that it would work) were made in my smaller compression apparatus; which is a massive iron cylinder, and very unwieldy for heating purposes. The results below are therefore strictly preliminary and provisional. The difficulty of knowing the interior temperature, and especially *of making sure that it has ceased to rise*, during the measures, renders them very dubious. I hope soon to supply more trustworthy results.

Temperature C.	Mean Difference of Readings of Pressure Gauge for the same three successive Wires.		
0°·5	14·5	16·7	17·7
8°·0	14·9	17·5	19·7
49°·0	24·6	22·0	21·8

If these at all represent the truth, they indicate a very curious result. But I draw no inference till I can repeat the work under more favourable circumstances.

PRIVATE BUSINESS.

The following Candidates were balloted for and declared duly elected Fellows of the Society:—Mr H. Bellyse Baildon, B.A.; Mr Robert Chambers; Dr Charles M'Bride.

Monday, 15th December 1884.

ROBERT GRAY, Esq., Vice-President, in the Chair.

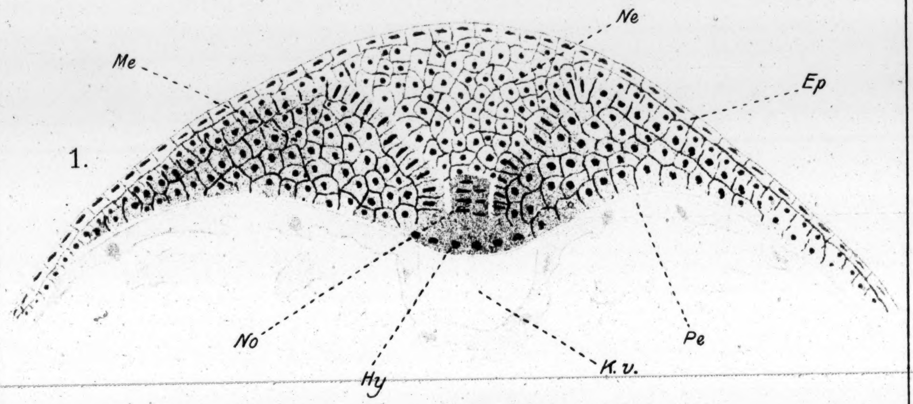
The following Communications were read:—

1. On the Theory of the Tides. Part I. By Edward Sang, LL.D.

2. On the Nature and Significance of the Structure known as Kupffer's Vesicle in Teleostean Embryos. By Mr J. T. Cunningham, B.A. (Plates I., II.)

The study of the development of the herring, of which the following pages contain the first results, was commenced at the beginning

1.



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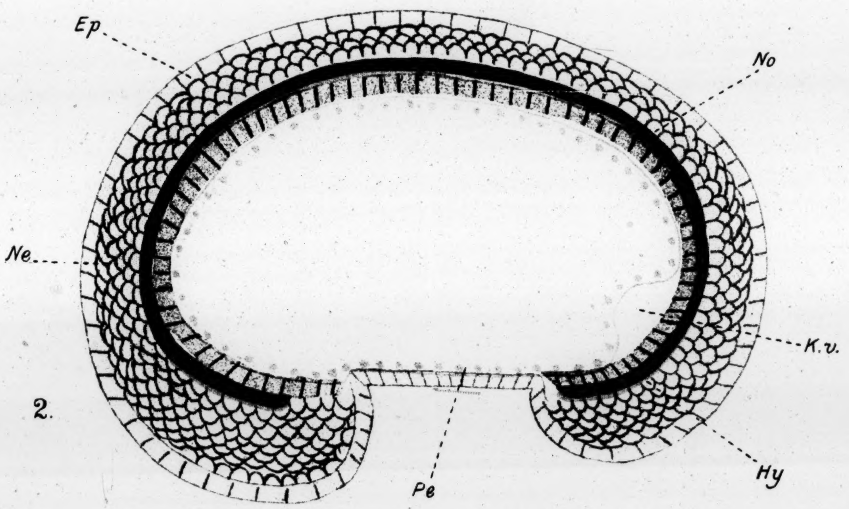

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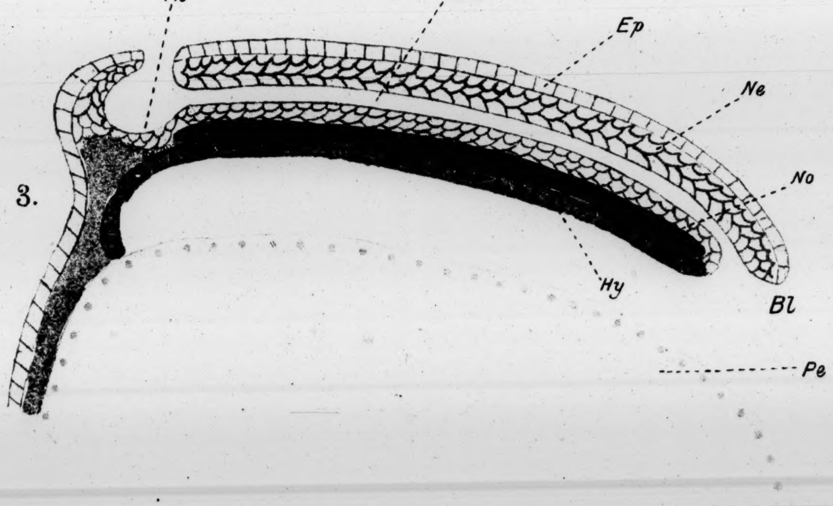

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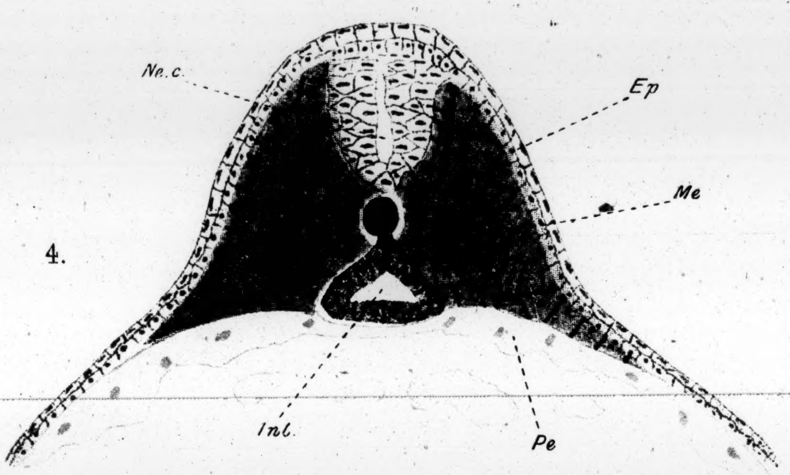

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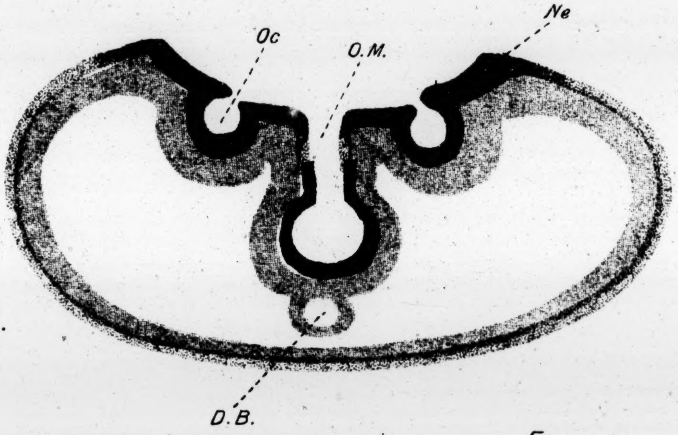

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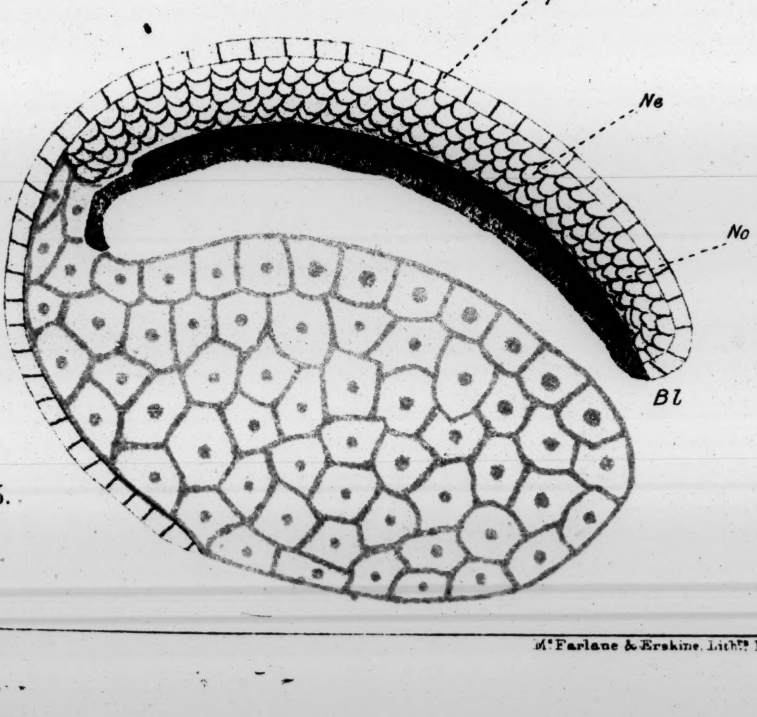

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of last August. At that time I went to a small fishing village called Sea Houses, North Sunderland, on the Northumbrian coast, and obtained a large number of herring ova, which I fertilised artificially on glass plates when on board a herring boat off the Longstone Lighthouse. I kept the spawn in wooden boxes sunk close to the shore, and was successful in keeping a proportion of them in a healthy condition up to the time of hatching, and for several days after. When I returned to the Scottish Marine Station on September 3, I brought with me about a dozen newly-hatched larvæ in a corked bottle of sea water, and some of these were alive on September 11, having lived over nine days. I attempted to feed the larvæ by putting in the water some of the small organisms taken by the tow-net, and those which I examined on 11th September had digested food in the intestine. I had previously experimented with herring eggs in August and September 1883, when I accompanied some members of the Scottish Fishery Board to the Moray Firth, to investigate the herring question there. In March of the present year also I obtained and artificially fertilised herring ova off Anstruther, in the Firth of Forth. On neither of these occasions had I suitable opportunities for making a fruitful study of my material. At North Sunderland I spent most of my time in observing the living embryos at succeeding stages, and preserving examples at frequent intervals. These preserved specimens I have cut into sections in the laboratory of the Granton Station since my return.

The rate of development in herring eggs, as in those of most Teleosteans, varies considerably with the temperature. The eggs which I studied were exposed to a temperature varying from 53° to 58° Fahr. (11°·5 to 14°·5 C.), and they hatched on the eighth and ninth days after fertilisation.

Kupffer's vesicle, as is well known, is a small globular cavity which appears at an early stage in Teleostean development between the posterior end of the embryo and the yolk. In the herring eggs with which I had to deal the structure was visible early on the third day, and remained visible for about eight or nine hours; on the fourth day it could not be seen. At the time of its appearance the yolk was entirely enclosed by the blastoderm, the two ends of the embryo almost met at the ventral side of the ovum, and the rudiments of the eyes and ears were formed.

Historical Summary.—The vesicle was first described by Kupffer[*] in 1868 in *Gasterosteus aculeatus*, *Gobius minutus*, and *Gobius niger*. He described it as lying beneath the intestine, stated that it was lined with cells, and considered that it was homologous with the allantois of the Amniota. In later papers Kupffer came to the conclusion that the vesicle was connected with the development of the urinary organs and ducts.

Balfour, in his *Comparative Embryology*, considered the vesicle as homologous with the post-anal vesicle in Elasmobranchs. He gave no reasons for this view, intending probably to investigate the point at some future time.

In 1880[†] M. Henneguy published, in the *Bulletin de la Société Philomathique de Paris*, some researches on the development of the Perch, in which he announced that he believed he had found an opening from Kupffer's vesicle to the exterior, and considered that the structure represented the primitive intestine of Cyclostomi and Batrachia, its opening being the anus of Rusconi. In 1883 a paper on Teleostean embryology, by Messrs Kingsley and Conn,[‡] appeared in the *Memoirs of the Boston Society of Natural History*. These authors had studied chiefly the pelagic ova of Ctenolabrus, one of the Wrasses. They trusted entirely to optical sections of the living embryo. They devote two short paragraphs to Kupffer's vesicle; they describe its origin from a number of small granules which coalesce, say nothing of its relation to the layers, though their figure is correct as far as it goes, and think that Balfour's view is much more probable than Henneguy's.

Finally, Professor A. Agassiz and C. O. Whitman have recently published some researches on the development of pelagic Teleostean eggs carried on at the Newport Marine Laboratory.[§] Here, again, ova of Ctenolabrus were principally studied. Kupffer's vesicle is dealt with in some detail; the description of its origin given by Kingsley and Conn is confirmed, if by granules are understood small spaces; the vesicle is correctly described as lying beneath the chorda and entodermic stratum, and as having no sort of connection with any tubular structure whatever. It disappears as the intestine is formed; but the authors have not recognised the fact that the periblast aids in forming the floor of the intestine, as I shall show below. They believe that a lumen exists in the part of the intestine formed in the region of the vesicle, from the time of the latter's disappearance onwards. It is very surprising that, having thus grasped clearly the relations of the vesicle, Agassiz and Whitman have not seen its meaning; they say that they are not ready to accept Balfour's interpretation, but the views of Kupffer and Henneguy are still more unsatisfactory. I hope to show that Henneguy's is the obvious and only morphological interpretation possible of the structure in question.

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[*] *Arch. f. mik. Anat.*, Bd. iv.

[†] *Ann. and Mag. Nat. Hist.*, ser. 5, vol. vi.

[‡] Vol. iii. No. vi., April 1883

[§] *Proc. Amer. Acad. Arts and Sciences*, vol. xx. Aug. 1884.

I may be allowed to explain that I arrived at the view I hold independently, not having seen any account of Henneguy's results till my opinion had been formed; at present I have only been able to see an abstract of Henneguy's paper, and do not know if his foundation of fact was more or less firm than my own.

Kupffer's vesicle, then, as seen in a successful series of transverse sections through a herring ovum at the proper stage, is a hemispherical cavity, bounded above by the lowest layer of the blastoderm, and laterally and inferiorly by the periblast. The latter is simply the outer layer of the yolk containing nuclei, but not divided into cells. In the herring at this stage the hypoblast cells are not distinctly differentiated from the mesoblast. The notochord is well formed, not yet vacuolated, and, as usual, not sharply marked off from the hypoblast. The neurochord is present as a thick cord of cells derived from the epiblast, and containing no canal (see Plate I. fig. 1).

It is clear, then, that Kupffer's vesicle has the same relation to the embryonic layers as the invagination cavity in Elasmobranchs, Amphibia, and Cyclostomi (Petromyzon). The vesicle has no differentiated cell-walls of its own; it is simply a depression in the periblast. The differences between its relations and those of the cavities with which I am comparing it are—first, its small extent, and second, the want of an opening to the exterior. As to the extent, the vesicle is a rudiment, a remnant of a larger cavity; as to its opening, it is on a par with all the other cavities in the Teleostean embryo. The neural canal, the cavity of the otocyst, the cavity of the crystalline lens, none of these arises in the Teleostean by direct invagination from the exterior, as they all do in Elasmobranchs. The neurenteric canal, as a distinct lumen, does not exist in Teleostean embryos, nor does the blastopore which would give rise to it. In Petromyzon we have an interesting condition intermediate between the Elasmobranch and the Teleostean. The neural canal does not arise as a groove open to the exterior, but, as in Teleosteans appears at a later stage closed from the beginning; while the gastrula cavity, on the other hand, is formed by direct invagination. The part of the blastopore represented by the neurenteric canal has disappeared, and not the part which in the Elasmobranch remains open to the exterior longest (see Plate I. figs. 2, 3; Plate II. fig. 5).

As far as actual comparison goes, therefore, there is every reason to believe that Kupffer's vesicle represents the gastrula cavity, or that part of it which is left after deducting the body cavity; but still further evidence is offered by the subsequent history of the vesicle. In Plate II. fig. 4 is shown the condition of the herring embryo on the fourth day in the region where Kupffer's vesicle existed. The intestine is completely formed; and it will be noticed that beneath the intestine the periblast is very thin, and contains no nuclei, though these are visible laterally beneath the mesoblast. It seems clear, then, that the floor of the intestine in this region has been formed by the differentiation of cells round the nuclei of the periblast, exactly as in Elasmobranchs the periblastic floor of the gastrula cavity is transformed into the floor of the intestine.

I think there is no room for doubt that the significance of Kupffer's vesicle is completely elucidated by the facts and comparisons I have thus given; it is the last rudiment of the invagination cavity in the Teleostean.

Remarks on General Vertebrate Morphology.—The view which obtains homologies between Vertebrates on the one hand, and Chaetopoda, Crustacea, and Insecta on the other, by inverting the latter, was suggested long ago by Geoffrey St Hilaire, when he said that Vertebrates were insects walking on their backs. But the distinct hypothesis that a Vertebrate is actually descended from an ancestor which agreed in almost every point of its anatomy with a modern Chaetopod, owes its origin and support in great measure to Dr Dohrn, director of the Zoological Station at Naples. St Hilaire's suggestion was related to this theory very much as Lamarck's notions on evolution to the "Origin of Species."

The correspondence between a Chaetopod turned over, and say a fish, is very complete. On the upper side (ventral in Chaetopod's natural position) is the double nerve-cord; so also in the fish. In the axis is the intestine. Above the intestine in the inverted Chaetopod is a blood-vessel, in which the blood passes from the head towards the tail (subintestinal vessel in natural position). So also in the fish—the dorsal aorta. In the inverted Chaetopod, below the intestine, is a blood-vessel in which the blood passes from the tail to the head (dorsal blood-vessel in the natural position). So also in the fish—(subintestinal vein, heart, ventral aorta).

Moreover, in the embryo of Chaetopod, Insect or Crustacean, the blastoderm is formed on the ventral surface, along which the primitive blastopore extends, though in most cases its actual opening is narrowed to a small posterior aperture, which becomes the anus. In the Vertebrate embryo the blastoderm is dorsal, and, as Sedgwick* has shown in a recent paper, and as Miss Johnson † has confirmed, there is evidence in the Vertebrate embryo of a fusion of the layers at an early stage along the median line of the blastoderm, indicating a primitive elongated blastopore.

Sedgwick's view of the morphology of Vertebrates differs from Dohrn's in one most important point. The former considers the actual mouth and anus of Vertebrates to be identical with the primitive mouth and anus, and supposes that a portion of the nervous system, representing the supraœsophageal ganglia of Chaetopods, &c., has disappeared. On Dohrn's view, the primitive anus was terminal, and is represented (when the actual blastopore is enclosed by the medullary groove) by the neurenteric canal. Where was the primitive mouth if the actual mouth is a secondary structure? Dohrn ‡ pointed out clearly in 1875 that a primitive mouth must have existed, surrounded, as in modern Chaetopods, by nerve-cord. He first supposed that the hypophysis cerebri, or pituitary body, then believed to be developed from the intestine, was the rudiment he was seeking; but in the publication I have referred to he definitely abandoned this view, and took up the theory that the sinus rhomboidalis in the medulla oblongata, the fourth ventricle in fact, was the position of the primitive mouth. This view he has also since given up. The hypothesis was discussed by Balfour in his monograph on the development of Elasmobranchs.*

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* *Q. J. M.*, January 3, 1884. † *Ibid.*, October 1884.

‡ *Ursprung der Wirbelthiere*, Leipzig, 1875.

It is an astonishing fact that, until the publication of Sedgwick's paper, morphologists had never fully grasped the significance of the development of the central nervous system in Vertebrates. Sedgwick has pointed out that the interior surface of the lining canal is part of the original dorsal surface of the body—that is to say, the condition through which an Elasmobranch or Amphibian embryo passes, in which the nervous system is a double cord of thickened epiblast, widening out anteriorly into a plate, was the permanent condition of the Vertebrate ancestor. Sedgwick has not followed out this fact to its ultimate consequences. He has been misled by his desire to consider the present mouth and anus as identical with the original structures. At the meeting of the British Association at York in 1881, Sir Richard Owen, in his address to the Department of Anatomy and Physiology of Section D, embraced the Dohrnian view of the homologies of Vertebrates and Invertebrates, and announced as a new hypothesis that the original œsophagus was represented by what he called the conario-hypophysial bract—that is, that the œsophagus originally passed from the hypophysis to the pineal gland. This was very much the same as Dohrn's first view before 1875, which had to be abandoned. The reasons why it is untenable are—first, the hypophysis is developed in most cases from the epiblast of the actual mouth, and not from the hypoblast; and, second (a reason which I believe has never before been definitely stated), that the original mouth did not open on what is now the dorsal surface above the nervous system, but on the floor of the cerebral vesicles, which is part of the primitive body surface. The conditions required for the rudiment of the primitive mouth are satisfied completely, and it seems to me exclusively, by the infundibulum. This latter structure is in the embryo a diverticulum from the floor of the first cerebral vesicle, which comes into contact with the anterior end of the mesenteron. In Miss Johnson's paper "On the Newt" a pit is described at the front end of the primitive blastopore, at the bottom of which pit epiblast and hypoblast are fused. The writer believes that this pit becomes the actual mouth. I think there is little doubt that if she had traced the fate of the pit in question she would have found that it became the infundibulum. I know that in herring embryos, in front of the notochord, the neurochord comes into contact with the hypoblast. Dohrn* has come to the conclusion that the hypophysis cerebri is the rudiment of a præoral pair of gill-clefts. If this conclusion hold good, it gives support to my view of the primitive mouth, for a pair of gill-clefts would have opened into the original œsophagus, and therefore might very well in actual Vertebrates come into connection with the rudiment of the primitive mouth, as the pituitary body does with the infundibulum. As for the pineal gland, it does not concern the question in the least; it is connected with the closing of the medullary canal, and is of comparatively small importance in the present discussion.

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* Professor Turner has kindly pointed out to me that John Goodsir, in a paper published in the second volume of his *Anatomical Memoirs*, Edinburgh, 1868, speaks of a view he once held concerning the Vertebrate primitive mouth, which is similar to the view once held by Dohrn, as mentioned above. Goodsir's view, originally published in the *Edinburgh Philosophical Journal* in 1857, was that the primitive œsophagus passed through the pituitary body, infundibulum, and third ventricle, and opened at the roof of the fourth ventricle behind the cerebellum. This theory, therefore, was a combination of the views which have since that time been successively favoured by Dohrn, and is anterior in date to any publications on the subject by Dohrn, Owen, or any others. Goodsir, in the paper I refer to, only mentions his theory to say that he abandoned it, because Reichert had shown that the pituitary body does not perforate the skull in the embryo. We know now that this reason for abandoning the theory does not exist. Goodsir completely agreed with Geoffrey St Hilaire's view concerning the reversed positions in Vertebrate and Annelid or Crustacean, and also recognised that the dorsal blood-vessel in the latter was homologous with the subintestinal vessel and heart in the Vertebrate.—*Note added Jan.* 28, 1885.

The Notochord.—Ever since the notochord was first described in some embryos as originating from the hypoblast, morphologists have considered that it must phylogenetically be derived from the intestine. Now, comparative anatomy shows that this supposition is untenable. The dorsal aorta in the fish is homologous with the subintestinal vessel in the Chaetopod: if the notochord had been derived from the intestine, it would lie between the aorta and the intestine—its actual position is between the aorta and the neurochord. It is not consistent with the principles of evolution to believe that it could first arise from the intestine, and afterwards pass into its present position. The notochord must have arisen phylogenetically from the mesoblast between the neurochord and the aorta. Some of my sections of herring embryos show the notochord distinctly marked off from the hypoblast, and continuous with the neurochord. This is not to be interpreted as showing that the notochord was derived from the neurochord, a skeletal structure from an epiblastic. The reason why the notochord is at first in close relation with the hypoblast, or with the neurochord, is that its development in the embryo has come to take place at so early a stage that the fusion of the three layers along the line of the primitive blastopore has not disappeared before the notochord appears. Balfour says, that in Elasmobranchs it is difficult to ascertain whether the notochord is derived from the hypoblast or is a central column of mesoblast cells. We thus get back to the old view, that the notochord is homologous with the three giant fibres beneath the nerve-chord in the earth-worm; both are mesoblastic structures, developed in the same position for the purpose of supporting the nerve-chord. It has been suggested that the typhlosole in the earth-worm represents the notochord; as I have shown this is impossible; the typhlosole reappears in the spiral valve of the intestine of Elasmobranchs and other fishes. The fact that the notochord stops short just behind the infundibulum is fully accounted for by my theory that the latter structure is the primitive mouth.

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* *Mitt. der Zool. Station zu Neapel*, Bd. iv. Heft. 1.

The Vertebrate Eye.—The peculiarity in the development of the vertebrate eye, as compared with that of Invertebrates, has long ceased to be so great a mystery as it was to earlier morphologists. In his address to the Department of Anatomy and Physiology of Section D, at the British Association meeting at Swansea in 1880, Balfour pointed out that the retina was formed from the floor of the brain, that is to say, from the same portion of the primitive epidermis which formed the central nervous system. In a similar way the rhabdoms or retinal elements in the eyes of Arthropods are formed from the supraœsophageal ganglia. In his little book on Degeneration in the *Nature* series, Prof. Lankester has inferred from the consideration of the cerebral eyes of Ascidians, that the original vertebrate ancestor was transparent, and had eyes on the floor of its brain; that when the animal became opaque the eyes gradually grew out to meet the epidermis at the sides of the head. But Sedgwick's revelation that the floor of the cerebral vesicle was once the surface of the head, enables us to go a step further, and form a clear conception of the state of the eyes in the primitive vertebrate ancestor, and the transition to the present state. In the worm-like ancestor at the sides of and somewhat in front of the mouth were a pair of eyes, formed as simple pits in the epidermal nerve area, exactly similar to the simple open eye-cups which exist in Patella and Haliotis at the present day. When the mouth and nerve area were covered by the formation of the neural canal the eyes became cerebral, and were influenced by light coming through the transparent tissues. In this condition the eye has persisted in Ascidians. In Vertebrates the body has become opaque, the eye-cups have grown out towards the side of the head, where a thickening took place in the epidermis to form the crystalline lens, which afterwards was pinched off and sunk in the mesoblast. We may regard the humours, the cornea, and the lens of the vertebrate eye as parts which have retained their primitive transparency (see Plate II. fig. 6).

We have now made more compact the inversion theory of vertebrate morphology. Sedgwick has attempted to go further, and account for segmentation gill-clefts, segmental organs, and abdominal pores, by deriving them from Coelenterates similar to Zoantharians. His theory is very ingenious, and for the most part extremely probable. But it agrees much better with the view that the infundibulum represents the primitive mouth, and the neurenteric canal the primitive anus, than with Sedgwick's own view concerning these points. For, according to Sedgwick himself, the planes of segmentation are perpendicular to the direction of the elongated blastopore, while if the present anus of a fish is the primitive anus, the segments of the fish's tail are parallel to part of the primitive blastopore. And if the primitive blastopore extended from the actual mouth to the actual anus in such a fish as a cod or blenny, it would occupy almost $\frac{9}{10}$ths of the longest circumference of the body. Balanoglossus, too, seems to give evidence that openings can arise from the intestine to the exterior independently both of segmental organs and blastopore. There is one other point which Sedgwick neglects, namely, that segments in all segmented animals are formed between the telson and the last segment. I am not aware that in any Coelenterate the mesenteries are formed according to this law.

EXPLANATION OF PLATES I. AND II.

In all the diagrams homologous layers are of the same colour.

Reference Letters.

Bl.	Blastopore.
D. B.	Dorsal blood-vessel in ancestral subintestinal in actual vertebrate.
Ep.	Epiblast.
Hy.	Hypoblast.
In.	Infundibulum.
Int.	Intestine.
K. v.	Kupffer's vesicle.
Me.	Mesoblast.
Ne.	Neurochord.
Ne. c.	Neural canal.
No.	Notochord.
O. m.	Original mouth.
O. e.	Original eye.
Pe.	Periblast.

Fig. 1. Diagram of a transverse section of a herring embryo passing through Kupffer's vesicle. The colouring of the layers is the only point in which this figure differs from the drawing of an actual section.

Fig. 2. Diagram of a section through the plane of symmetry of a herring embryo at the same stage. This figure is drawn after the study of a series of transverse sections.

Fig. 3. Diagram of a section through the plane of symmetry of an Elasmobranch embryo when the blastopore and medullary groove are being closed.

Fig. 4. Diagram of a section of a herring embryo through the region where Kupffer's vesicle has given place to the intestine.

Fig. 5. Diagram of a section through the plane of symmetry of an embryo of Petromyzon, in which the gastrula cavity exists as in Elasmobranchs, but the neural canal is at first absent, as in Teleosteans.

Fig. 6. Diagram of an ideal section of the Vertebrate ancestor; the section is oblique, and passes through both eyes, and the original nearly terminal mouth, which in actual Vertebrates is represented by the infunbibulum.

3. The Relations of the Alveolar form of Cleft Palate to the Incisor Teeth and the Intermaxillary Bones. By Prof. Wm. Turner, M.B., F.R.S.

Dr Albrecht of Brussels has recently traversed the well-known and generally accepted theory originally advanced by Goethe, that in the alveolar form of cleft palate the fissure lies in the plane of the suture between the intermaxillary and superior maxillary bones, and has suggested, in substitution for this theory, that in the early embryo each intermaxilla is divided into an inner mesial bone (endognathion) and an outer lateral bone (mesognathion), and that the alveolar cleft is an open state of the suture intervening between these two divisions.

In this communication the author discussed Albrect's theory and described the observations which he had made (A) on casts of the roof of the mouth in fifteen cases of cleft palate, and (B) of hard palates where there was no cleft, with the view of ascertaining their bearing on the theory advanced by Albrecht.

A. For the opportunity of examining these casts, and for information regarding the cases, he has to express obligations to his colleagues, Professors Annandale and Chiene; to Dr John Smith, President of the Royal College of Surgeons, Edinburgh; Dr Joseph Bell, Senior Surgeon, Edinburgh Royal Infirmary; W. Bowman Macleod, Esq., L.D.S., Dean of the Dental School; and Andrew Wilson, Esq., L.D.S., Lecturer on Dental Anatomy, Edinburgh.

He has also analysed Th. Kölliker's account of forty-nine preparations of cleft palate examined in several museums in Germany.

Dr Kölliker's forty-nine preparations and the fifteen casts examined by the author, make in all sixty-four specimens in which the relations of the alveolar cleft to the teeth have been definitely observed. These specimens resolve themselves into two groups—*a*, one in which no precanine tooth intervened between the canine and the cleft, and in this group were thirteen specimens; *b*, one in which a precanine was situated between the canine and the cleft, and this consisted of fifty-one specimens. Obviously, therefore, much the larger number of persons with the alveolar form of cleft palate possess a tooth in front of the canine, which is cut off from the incisor series of teeth by the gap in the border of the jaw.

The author then discussed the homologies of the incisor teeth with reference to the intermaxillary bones, and the nature of the precanine tooth. He believed that the precanine should be regarded as In ^{3}, and that the missing incisor in the normal human dental arcade is In ^{2}. Whilst agreeing with Th. Kölliker in his criticism that the teeth and jaws arise quite independently of each other, and only become related secondarily, yet from the fact that in so large a proportion of the casts a tooth, which from its position must be referred to the incisor series, was situated between the canine and the cleft could scarcely be without some significance, and from the frequency of its occurrence, should not be regarded as a mere accidental displacement of a tooth germ. The very frequent presence of a precanine tooth may therefore be regarded as supporting Albrecht's view of the position of the cleft.

Albrecht has, however, had the advantage of examining several skulls in which the alveolar cleft was seen to separate the intermaxilla into an inner and an outer part, each carrying its appropriate incisor or incisors. In addition to the skulls of the horse and the calves referred to in his first essay on this subject, he has since described and figured an adult human skull in the University of Kiel, in which a right cleft existed in the corresponding intermaxilla, and the right maxillo-intermaxillary suture coexisted with and was quite distinct from it; a new-born infant with double cleft, in which the same suture was present; the jaw of a child about one year old, in the museum at Ghent, in which, with a left cleft in the corresponding intermaxilla, a left maxillo-premaxillary suture was present. In all these cases the part of the intermaxilla which was situated outside the cleft contained the socket for the precanine incisor. The anatomical evidence will therefore justify the statement that, in a proportion of cases of alveolar cleft palate, the cleft lies within the intermaxilla, the cleft coexisting with the maxillo-intermaxillary suture, and an incisor tooth is situated in the interval between the cleft and the canine of the same side.

In the group of cases of alveolar cleft in which no precanine tooth intervened between the canine and the cleft, it is not improbable that one may find examples of a cleft occurring in the plane of the maxillo-intermaxillary suture, and not within the intermaxilla itself. Wherever a suture exists, there, of course, a possibility of an imperfect union of the two bones may arise. Should the bones remain separate, and should the want of union be accompanied by a non-closure of the superjacent soft parts, then the imperfect development would lead to the production of a cleft in the alveolar region and in the upper lip, and the theory of Goethe might therefore be applicable to such cases.

The author then referred to the descriptions of Leidy, Callender, and Th. Kölliker on the development of the intermaxillary bones.

B. The object of examining the hard palate where there was no cleft was to ascertain if any suture, or the remains of a suture, could be seen in its anterior part, immediately behind the incisor teeth, to indicate that, as Dr Albrecht contends, the incisive or intermaxillary element of the human upper jaw had originally consisted of two bones on each side, a mesial or internal, and a lateral or external. The author described six specimens, in each of which a narrow fissure was present in each intermaxilla, which apparently represented the remains of an intra-incisive suture. It existed in conjunction with a pair of maxillo-intermaxillary sutures.

The place of origin of the intra-incisive fissure in the majority of the specimens was from the maxillo-premaxillary suture external to the incisive canal, which canal, therefore, on the theory that the intermaxilla consists of an inner and an outer division, would lie in relation to the inner division close to the articulation between it and the superior maxilla. In one instance the intra-incisive fissure penetrated into the naso-palatine canal. Obviously, therefore, some condition arising during the development of the bone determined the origin and direction of the fissure in question.

What is yet wanted, however, in order to give completeness to the evidence of the division of the intermaxillary bone into an inner and an outer part, is the discovery that the intermaxillary bone normally arises from two distinct centres of ossification, one for the inner, the other for the outer part. Of this there is at present no evidence. But, in connection with this matter, it ought not to be forgotten that it is only recently that the embryological evidence of the origin of the intermaxillary part of the human upper jaw, from a centre distinct from that of the superior maxilla, has been completed. And yet for nearly a century, on such minor evidence as was advanced by Goethe, viz., the suture on the hard palate extending through to the nasal surface, anatomists have believed and taught that the human upper jaw represented both the superior and intermaxillary bones in any other mammal. Where a question in human embryology hinges upon an examination of parts in a very early stage of development, we often have to wait for many years before an appropriate specimen falls into the hands of a competent observer.

This paper will appear *in extenso* in the *Journal of Anatomy and Physiology*, Jan. 1885.

4. Apparent Lines of Force on passing a Current through Water. By Thomas Andrews, F.R.S.E., F.C.S., Wortley Iron Works, near Sheffield.

The author has recently been engaged in a variety of electrical observations in connection with wrought iron and steels, in the course of which it occurred to him to investigate some of the effects produced in the presence of nascent oxygen, of the ozone type, by a current of sufficient strength to readily decompose these metals by oxidation whilst immersed as electrodes in distilled water, the great resistance of the latter apparently facilitating the interesting results observed and recorded below. An average of ten determinations gave the resistance of the distilled water used as 48,234 ohms, and in the following experiments, the current from forty small Leclanché cells was employed. The current was passed through distilled water contained in a beaker (the distilled water was tested for impurities, but was found free from everything except the faintest trace of chlorides), using two round electrodes of the same bright metal $\frac{297}{1000}$ inch diameter by 2 inches long, suspended as shown in the sketch on page 20 (2 inches apart). Hydrogen was rapidly given off at the kathode B, whilst there, at first, slowly exuded from the anode metal A, faint clouds accompanied by long fine streaks of ferric hydrate, which gradually increased in quantity until in about fifteen minutes the whole of the water presented a yellowish turbid appearance, and flocculent masses of oxides began to subside (these were of course reduced to protoxide near the evolved hydrogen from the other electrode B). The circumstance, however, to which the author desires to call attention is the seeming indication of lines of force, during the electrolytic disintegration. The film of oxides of iron (commencing to exude from the electrode A) appeared gradually to arrange itself into innumerable delicately curved filaments or streaks, slowly extending in a general horizontal direction from one electrode to the other. When the bars were inclined towards each other, the filaments emanated in greatest quantity from the nearest point, as indicated in the sketch. After the current had passed some ten minutes, these became increasingly numerous, and were seen proceeding from most parts of A. After a time, however, they became so interwoven and complicated in character as ultimately to present the appearance of thick streaky clouds, composed of finely divided oxide of iron, but yet retained the distinct aspect of a broad current of this nature, extending itself towards the other electrode B.

A pair of electrodes of the same size and shape were tried of each of the following metals from the author's standard samples (see table A), when similar lines of force were observed, though these indications were perhaps most noticeable when employing cast metal electrodes. The current was passed in each case for a total period of sixty-five minutes. Every bar was cleaned, dried at 212°,

TABLE A.

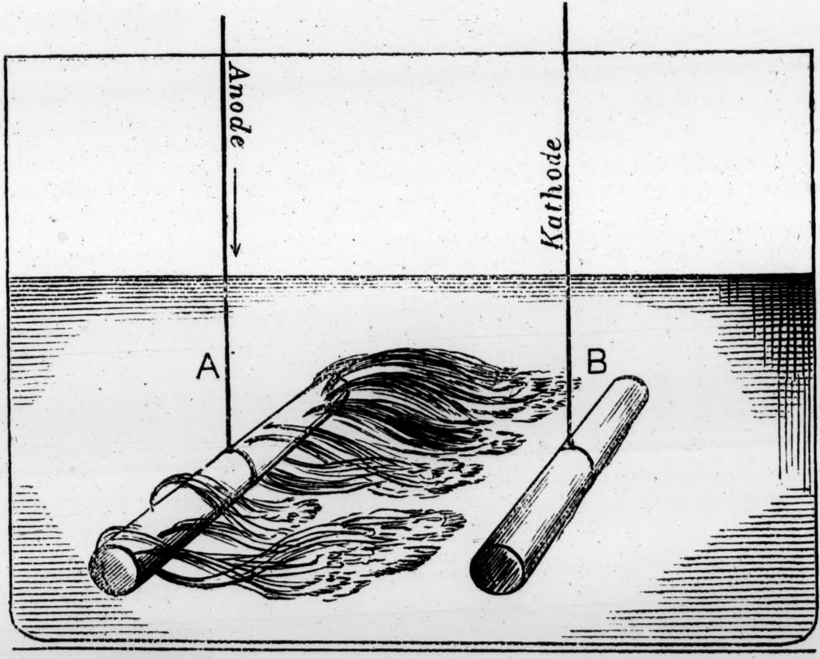
Metals used as Electrodes.	Percentage of combined Carbon.	Percentage loss in weight of the Metal forming the Anode for 65 minutes in Distilled Water.	Percentage loss in weight of the Metal forming the Anode for 65 minutes in Sea Water.
"Soft" Siemens-Martin steel, . . .	..	...	...
Wrought iron, . .	Trace	0·08	0·14
"Soft" cast steel, .	0·570	0·09	0·19
Bessemer steel, . .	0·550	0·09	0·20
Puddled steel, . .	0·440	0·07	0·19
Puddled steel (chilled), .	0·490	0·03	0·22
"Hard" cast steel, .	1·600	0·08	0·25
Cast metal, Graphitic carbon 2·400	1·000	0·10	0·26

and weighed on the balance before and after each experiment, with results given on table A. The kathode metals lost nothing in weight.

In the case of distilled water, the loss was from pure oxidation in presence of the nascent oxygen.

The generally curved contour of some of the outer streaks was doubtless consequent on the mode of emission of the current from the round surface of the electrode, influenced by the resistance effects of the distilled water.

The sketch is from careful drawings, made at the time of observation, and gives a fair representation, though the lines were perhaps not always so well defined. These effects were obtained when using distilled water (possibly owing to its very great resistance); when the salt water was substituted the curved streaks were not easily distinguishable, to some extent owing probably to the greater, and more rapid oxidation, and the much smaller resistance. The mixed ferrous and ferric oxides in this minute state of subdivision appeared to manifest magnetic properties, the polarised particles acting in magnetic fashion



arranged themselves in the above manner, under the influence of the current, and indicating these apparent lines of force.

The curves presented such an interesting appearance that the author thought it desirable to record the observation.

5. Note on a Theorem of Clerk-Maxwell. By Prof. Tait.

At the last meeting of the Society Sir W. Thomson again raised the question of the validity of Boltzmann's Theorem, to which I had called attention two sessions ago. He expressed, at the same time, some doubts as to Clerk-Maxwell's Theorem (of which Boltzmann's is an extension); doubts, however, confined to the proof given by Maxwell, not as to the truth of the theorem itself. This theorem is the extremely important one, that in a mixture of two kinds of particles the average kinetic energy of the particles of each kind is the same. The proof, as given in the *Philosophical Magazine* for 1860, is so very condensed as rather to surprise the reader by the extraordinary rapidity with which it seems to show that the final average is attained. I have, therefore, expanded it so that the nature of the approximation to the average may be clearly traced.

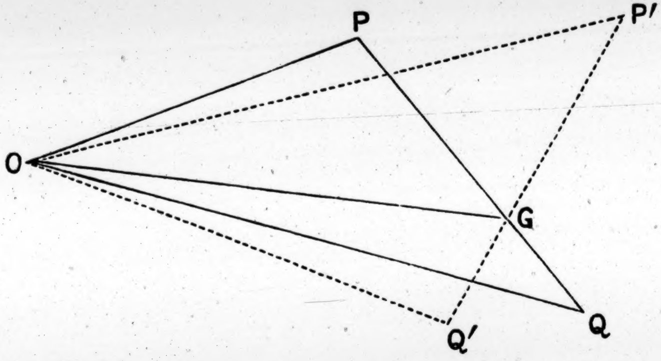
Lemma.—The mean value, of the square of the distance of any point on a sphere from an internal or external point A, is the sum of the squares of the radius of the sphere and of the distance of A from the centre.

The proof is immediate. Divide the spherical surface into pairs of elements by double cones, of very small angle, whose vertices are at the centre. For each pair of these the theorem is obviously true.

Hence if the speeds of two points be p and q, their mean square *relative speed* is $p^2 + q^2$.

Clerk-Maxwell's Prop. VI.

The following figure shows points on a sort of hodograph. Let OP represent, in direction and magnitude (p), the velocity of a particle of mass P. Similarly OQ that of Q, speed q. Let $\angle POQ = a$. Let G be the centre of inertia of P and Q supposed placed at these points in the figure. After the impact G remains undisplaced, but PGQ becomes P′GQ′ where GP′ = GP, GQ′ = GQ, and (by a previous proposition) all directions of P′Q′ (in space) are equally likely. Let the speed OP′ be called p'; OQ′, q'; and let $\angle OGP' = \theta$.



Then
$$OG^2 = \frac{P^2p^2 + Q^2q^2 + 2PQpq\cos\alpha}{(P+Q)^2},$$

also
$$Pp'^2 - Qq'^2 = P(OG^2 + GP^2 - 2OG\,.\,GP\cos\theta) - Q(OG^2 + GQ^2 + 2OG\,.\,GQ\cos\theta)$$

$$= (P-Q)\,OG^2 + \frac{PQ(Q-P)}{(P+Q)^2}\overline{PQ}^2 - \frac{4PQ\,.\,OG}{P+Q}\overline{PQ}\cos\theta$$

$$= \frac{P-Q}{(P+Q)^2}\left(P^2p^2 + Q^2q^2 + 2PQpq\cos\alpha - PQ(p^2+q^2-2pq\cos\alpha)\right) - \frac{4PQ\,.\,OG}{P+Q}\overline{PQ}\cos\theta$$

$$= \left(\frac{P-Q}{P+Q}\right)^2\left\{Pp^2 - Qq^2 + \frac{4PQpq}{P-Q}\cos\alpha\right\} - \frac{4PQ\,.\,OG}{P+Q}\overline{PQ}\cos\theta.$$

Hence, taking mean values, with regard to α, to θ, and to the plane through OG in which θ is measured (which, of course, presupposes an immense number of impacts), we have

$$Pp'^2 - Qq'^2 = \left(\frac{P-Q}{P+Q}\right)^2(Pp^2 - Qq^2),$$

which is Clerk-Maxwell's result. The above investigation, however, shows the somewhat complex process of averages by which it is obtained.

The Corollary, in which theproposition is extended to a number of systems of different sets of particles, follows in the same way;

but it specially requires the postulate (not, explicitly at least, alluded to in Maxwell's paper), *that every pair of particles, to whichever set or sets they may belong, shall be perfectly free to collide.* It does not follow that the extension would hold in cases where there is any limitation to the freedom of collision,—such as is almost certainly the case when each particle of a gas is treated as a group of particles, or as a system with a considerably greater number of degrees of freedom than a free particle has. For some of the constituents of each particle may necessarily be so situated as never to encounter the corresponding constituents of another particle.

In fact Boltzmann's generalisation of Clerk-Maxwell's Theorem, in which he asserts that ultimately the energy is equally shared among all the degrees of freedom, would seem to be at once confuted by the collisions of smooth spheres, where each has three degrees of (rotational) freedom to which no energy at all is communicated.

6. Extraordinary Occurrence at House No. 7 York Place.

(*The following notice was sent to the General Secretary, from the Office of Messrs Hunter, Blair, and Cowan, W.S.*)

An occurrence of an extraordinary nature took place in the kitchen of this house on Monday evening, the 8th inst. The kitchen is in the area, and whilst the office-keeper and his wife and servant girl were seated in front of the fire, suddenly, about twenty minutes past eight, a terrific rumbling sound was heard in the chimney. Fearing that something was about to topple about their ears, they all sprang aside, and no sooner had they done so than a large sheet of flame issued from the chimney, and without disturbing the ashes in the grate, or touching the grate itself, swept close past the office-keeper, who was standing nearest to the fire-place, and extinguished the gas. Upon the gas being relit, an extraordinary state of matters was revealed. The apartment was filled with smoke and dust, while the brick wall partition opposite the fire-place, and which would be 12 to 14 feet distant from it, was so greatly injured that had it not been for the shelving with which it was lined in front, and which held it together, it would have fallen right out. As it was it stood greatly off the perpendicular, and was cracked and wrenched from its holdfast along the ceiling, and at one part it was bulged out as if it had been forcibly struck by some soft heavy bulky article. There was no appearance of scorching, as is generally the case when any object of an inflammable nature is struck by lightning. The vent, which is swept at regular intervals, was swept shortly before, and was therefore comparatively clean. There is no iron bracketing at the top of the chimney. With the exception of the partition, no other article in the kitchen was injured, or apparently touched. The gas pipe runs across the roof or ceiling from the opposite wall towards the fire-place, and the pendant is about the centre of the apartment, rather nearer the fire-place than otherwise, but neither received any injury, although the gas was extinguished. The evening was wet and boisterous, the wind having been from the west or north-west, but a little before the occurrence the rain had passed off, and later on it became quite clear, although there was evidently a good deal of moisture in the air.

7. Remarkable Sunrises on December 6th, 8th, and 11th, 1884. Described by A. E. M'Intosh, Esq.; Lady Mary Baillie of Polkemmet; and Patrick Dudgeon, Esq.; in letters to Alexander Buchan, Esq.

The Gardens, Lews Castle,
Stornoway, 11*th December* 1884.

Mr Buchan—Dear Sir,—On Saturday last, the 6th, at half-past 8 A.M., was observed here a very curious phenomena in the sky, due south and about 45° from the earth; it looked like oil or tar poured on water, and had all the colours of the rainbow, pink and blush being in the majority. It was observed here for about twenty-five minutes half an hour before daylight. I heard two peals of thunder, but saw no lightning, though we had lightning on Friday night. Saturday morning was fair, and I thought we were to have a fine day, but at noon it commenced raining, and continued until late at night. This morning (Thursday) I observed exactly the same phenomena and at the same time, and continued the same length of time. The morning was beautiful, but commenced at noon to rain, and is raining still (8 P.M.), but not heavy. Was this observed at Edinburgh? I must mention that it was seen to-day E.S.E., and I heard no thunder to-day. Please let me know how many months you have received of this year (the Reports I mean).—I am, dear Sir, yours truly,

A. E. M'Intosh.

Polkemmet,
Whitburn, 11*th December* 1884.

Dear Mr Buchan,—I wonder if any one has seen or noticed the wonderfully beautiful prismatic-coloured clouds which I have seen here on the mornings of the 8th and 11th. *This* morning was especially glorious about 8.30, rather a clear sky, with heavy brown clouds on the horizon, and some scudding along at a great pace. In the rifts behind these clouds, these white clouds appeared,—some like a mountain range tipped with colours,—one enormous oblong shape formed itself into a B, with pale blue in the middle; the principal one in the zenith was a flat heart shape, the edges brightly *defined* with the prismatic colours, and inside a gorgeous lake colour, *i.e.*, crimson. Sir William and I both saw it, and were amazed. You might like to take a note of this for future use.—I am, yours truly,

Mary Baillie.

Cargen,
Dumfries, 11*th December* 1884.

My Dear Sir,—I enclose a rough sketch of a beautiful and, I think, it must be a remarkable phenomenon which I observed this morning. I believe the same thing was seen on Monday morning about the same time, 9 to 9.30 a.m., but I did not observe it. The colours were remarkably fine, just like large sheets of fine mother-o'-pearl, as a lady observed when she saw them. The light spaces were almost clear of cloud apparently, and the sun was nearly behind the coloured spaces. I would like to know if you have observed the phenomenon in the country before. Weather very unsettled.—Believe me, yours truly,

Patrick Dudgeon.

Monday, 5th January 1885.

EDWARD SANG, Esq., LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. Anatomy and Physiology of *Patella vulgata.* Part I.—Anatomy. By R. J. Harvey Gibson, M.A. Communicated by Professor Herdman, D.Sc.

(*Abstract.*)

This paper forms the first half of Part I. of a research into the minute anatomy, physiology, and development of *Patella vulgata.* The paper aims at giving a complete account of this form in both its physiological and morphological aspects. It treats, first of all, with the external features and rough anatomy. The epidermal system is then treated in detail. Some interesting modifications of the superficial epithelium are described. The epithelial cells are usually elongated columnar, with processes connecting them with a dense subepithelial layer of connective tissue. The attachment of the circular muscle is covered with modified epithelium, the epiblast cells not being absent in that region, as is said to occur in the case of the adductor muscles of *Anodon.* The modified epithelium at one spot near the base of the tentacle functions as an eye. The cells are there pigmented, and the cuticle is modified so as to form a double layer, with intervening trabeculæ. The retinal epithelium springs from a nerve plexus, which is supported by a mesh-work of connective tissue fibres, with large lacunæ.

The arrangement of fasciculi in this muscular system points unmistakably to the attachment of *Patella* to a rock, being the result of muscular contraction and formation of a vacuum beneath the foot.

The muscle fibres are non-striped, save in the heart.

The gills are morphologically processes of the ventral surface of the mantle, which is itself physiologically an accessory respiratory organ. The epithelium is in the mantle and gills columnar, and the cells are not close to each other, thus permitting of gaseous exchange between them.

Pactile papillæ, over one hundred in number, are found situated at intervals along the edge of the mantle skin. They lie in pockets, which open on the ventral side of the skirt. They are apparently modifications of those found in *Haliotis*, and may serve as organs of locality-sense.

The observations of Lankester, Bourne, and Cunningham on the renal system are confirmed, and in some respects extended. The history of the renal epithelium appears to be similar to that stated by Von Jhering, to be characteristic of the cells of the kidney of *Tethys*. The structure and behaviour towards reagents of the epithelium of the right and left kidneys is different, and it is suggested that the excretion of the two kidneys may be chemically distinct.

The structure of the heart (auricle and ventricle) and blood-vessels, and the lacunar system, is discussed. The blood-corpuscles are colourless, and amœboid. Coagula resemble plasmodia of *Monobia*.

The alimentary, nervous, and reproductive systems are reserved for a future paper—the second half of Part I.

2. A Theory of Solution. By W. W. J. Nicol, M.A., B.Sc.

(*Abstract.*)

This paper contained an account of the experimental evidence in support of the theory of solution enunciated in a paper* "On the Nature of Solution," communicated to the Society in January 1883.

Solution of a salt in a liquid is a consequence of the attraction of the molecules of the liquid, for a molecule of the salt (*adhesion*), exceeding the attraction of the molecules of salt for one another (*cohesion*). As the number of dissolved salt molecules increases, the attraction of the molecules of liquid for the molecules of salt becomes more and more balanced by the mutual attraction of the salt molecules. When these two forces are in equilibrium, saturation ensues.

Increase of solubility with rise of temperature is due to the *cohesion* of the salt being diminished by heat to a greater extent than the *adhesion* of the liquid to the salt; and, conversely, diminished solubility with rise of temperature is due to the *cohesion* being less affected by rise of temperature than the *adhesion*.

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* *Phil. Mag.*, February 1883; see also *Proceedings R.S.E.*, 1880–82; *Berichte der deut. Chem. Ges.*, October 1883.

Evidence in support of the above is obtained from experiments on the following physical properties of salt solutions:—

Contraction on dilution.*
Coefficients of expansion.†
Molecular volumes.‡
Boiling points.§
Saturation point.||

Further, the solution of two salts in water is governed by the same laws as that of a single salt (when no double decomposition is possible): the solubility of both salts being increased in most instances.¶ When double decomposition is possible, then the amount and nature of it is conditioned by the solubility of the possible salts and the strength of the solution.**

The molecular volume of a salt in solution is a quantity made up of two *constants*, one for the metal, and another for the acid-radical. It follows from this, that the replacement of one metal by another in combination with an acid-radical is always attended by the same change in the molecular volume of the solution, or generally,

$$(M - M')R = C'$$
$$M(R - R') = C'.$$

where M and M′ are two different metals and R and R″ two acid-radicals.††

The presence or absence of *water of crystallisation* has no effect on the above law: it therefore follows that it has the same volume in solution as the solvent water. *Water of constitution*, however, shows itself in solution by possessing a volume markedly different from water of solution.‡‡

Finally, when the molecular volumes of a series of solutions of

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* *Phil. Mag.*, February 1883; *Chem. Soc. Journal*, March 1883.

† *Phil. Mag.*, February 1883.

‡ *Ibid.*

§ *Ibid.*

|| *Ibid.*

¶ *Phil. Mag.*, June 1884.

** *Ibid.*

†† *Berichte de deut. Chem. Ges.*, 1884.

‡‡ *Loc. cit.*, also *Phil. Mag.*, September 1884.

different strengths of the same salt are expressed by the empirical formula,

$$MV = 1800. + n\alpha + n^2\beta - n^3\gamma.$$

Where α, β, γ are three constants and n the number of salt molecules per 100 H_2O, it is found that $\frac{\beta}{\gamma}$ is equal to twice the solubility,—saturation is therefore reached when the further addition of a molecule of salt would produce a *diminution* of the mean molecular volume of the molecules already present. The last molecule added enters into solution with a volume sensibly equal to the above mean volume as is shown by the following equation,

$$(n\alpha + n^2\beta - n^3\gamma) - \left((n-1)\alpha + (n-1)^2\beta - (n-1)^3\gamma\right) = \alpha + n\beta - n^2\gamma.$$

when

$$n = \frac{\beta + \gamma}{2\gamma}.$$

3. On the Salinity of the Water in the Firth of Forth. By Hugh Robert Mill, B.Sc., F.C.S., Scottish Marine Station, Granton, Edinburgh. (Plates III., IV., V.)

Contents and Summary.

I. 1.—**Method of collecting Water Samples** from the surface and from various depths, with a description of the manner of making the neccessary observations, *e.g.*, *temperature, colour and transparency of water, and note-taking.* Illustration of Buchanan's slip water-bottle, pp. 30–34.

2.—**Determination of the Density.**—Description of the *hydrometer* and the manner of using it, and of *calculating* the results. Detail of experiments to determine the *error of the method;* each determination of density being in duplicate supplied data for this purpose. *Probable uncertainty* of density shown to be +0·00005, and that the method is capable of improvement, pp. 35–41.

3.—**Determination of Alkalinity.**—Method employed, Dittmar's modification of Tornøe's. *Accuracy* attained shows a deviation of 1·8 per cent. of carbonic acid found (total carbonic acid found being about 0·005 grammes per 1000·000), pp. 41–44.

II. 1.—**Notes of previous Work in Estuary Analysis.**—*Murray* in 1816, and *Davy* in 1843–44 on the Firth of Forth ; *Macadam* in 1855 on the Clyde and in the Beauly and Cromarty Firths in 1866 ; *Kyle* on the *Plate* in 1874 ; and the *German and Danish Commission* for the Investigation of the German Seas, on the Baltic, Skager Rack, and Cattegat, 1877–1881, pp. 44–46.

2.—The Geography of the Firth of Forth, illustrated by a chart, pp. 46, 47. (Plate III.)

3.—Observations on the Surface Salinity of the Firth.—Particulars of four *water-sampling trips* between Alloa and the Isle of May, illustrated by five curves (A to E). Effects of the flooded state of the rivers in November. *General result*, the salinity increases very rapidly from Alloa to Queensferry, less rapidly to Inchkeith, and very gradually to the Isle of May, where it decreases slightly. The effect of Tide, pp. 48–54.

4.—Surface Water Density at the Marine Station.—Density varied slightly during July, August, September, and October, considerably in November, and greatly in December. Details given, and effects traced to the different course of the Almond at high and low water, pp. 54–59.

5.—The Bottom Water of the Firth.—Comparison of bottom and surface water at six places on the Firth. Result that the difference is greatest (tides considered) at Grangemouth, and decreases steadily to the Isle of May. Particular case of difference between surface and bottom densities at Inchgarvie, pp. 59–61.

6.—The Alkalinity of the Firth.—Particulars given of 98 observations. General conclusion, that the alkalinity is roughly proportional to the density, but subject to considerable variations in the ratio. Alkalinity of bottom water, pp. 61–64.

It is the purpose of this paper to state, with some detail, the methods employed for examining the salinity and alkalinity of estuary water at the Scottish Marine Station at Granton; to give the probable uncertainty of these methods; and to describe and record six months' observations of the water of the river and Firth of Forth, up to December 31, 1884.

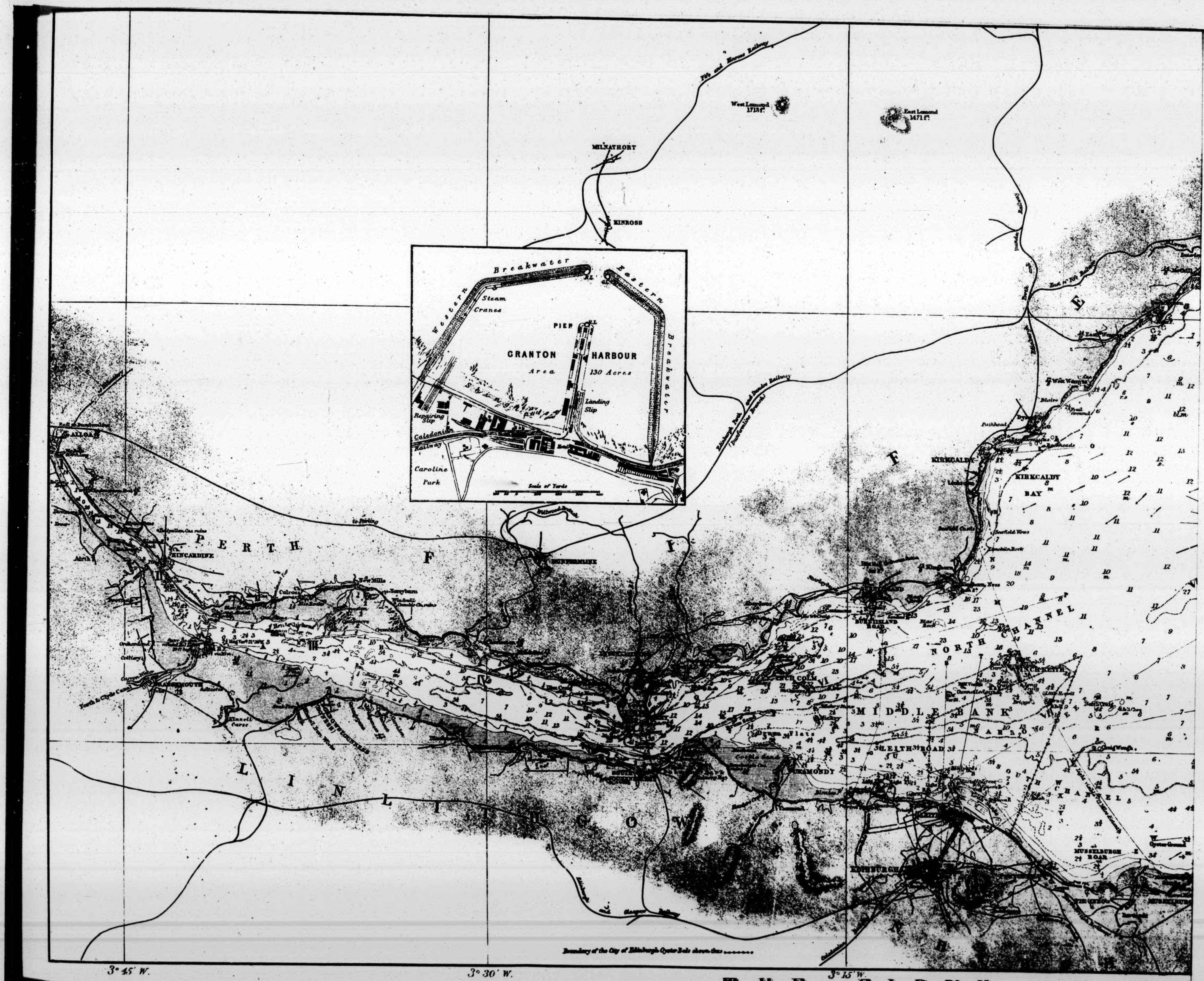
I. Methods.
II. Results of Preliminary Work.

I. Methods.

1. *Collection of Water Samples.*

To collect a sample of surface water from a small boat it is sufficient to wash out the bottle with the water, and then hold it a few inches under the surface until it fills. The temperature of the water is taken by means of an ordinary thermometer in a copper case.

On board a larger vessel the same thing may be done, the bottle being attached to a sounding line and lowered over the side; but in the work carried on on the Firth of Forth it is desirable not to



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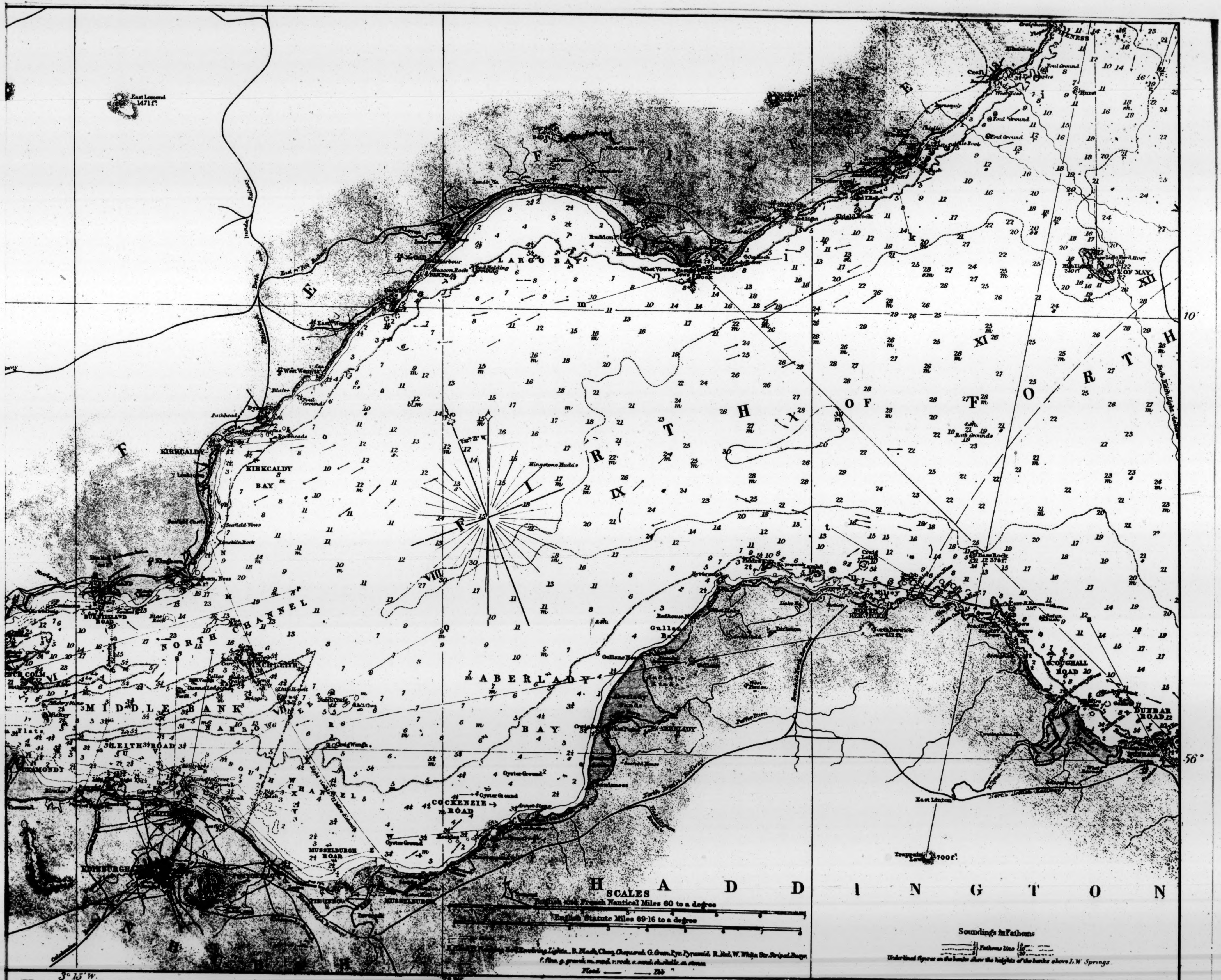
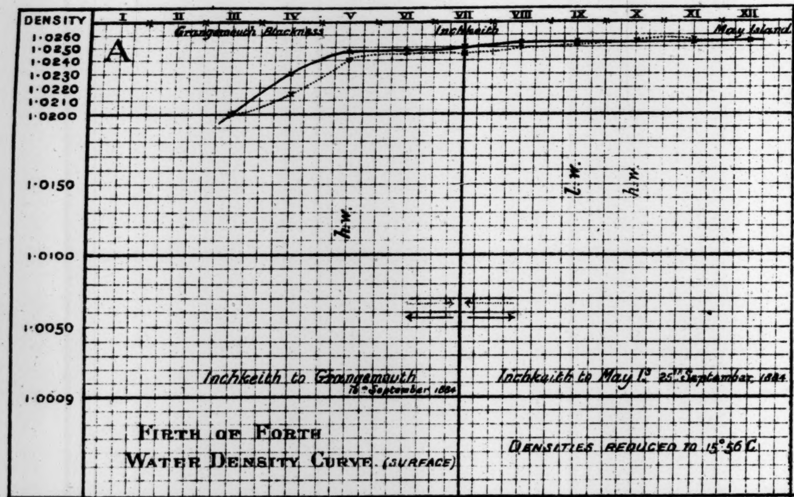
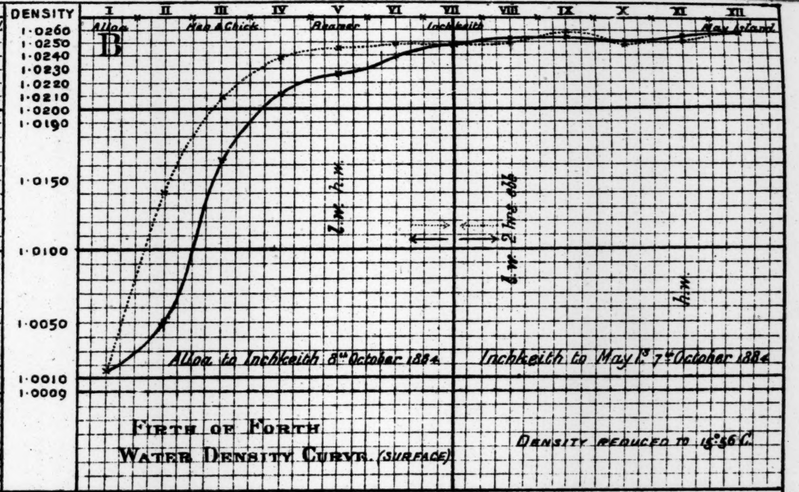


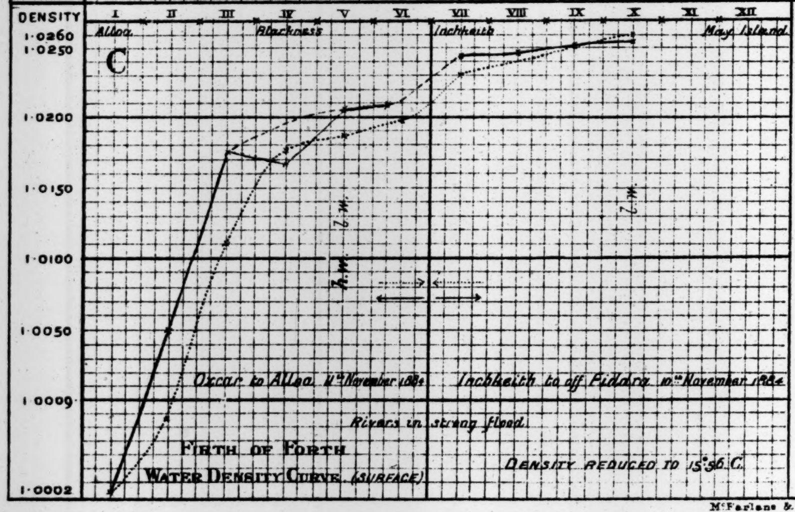
E FIRTH OF FORTH.

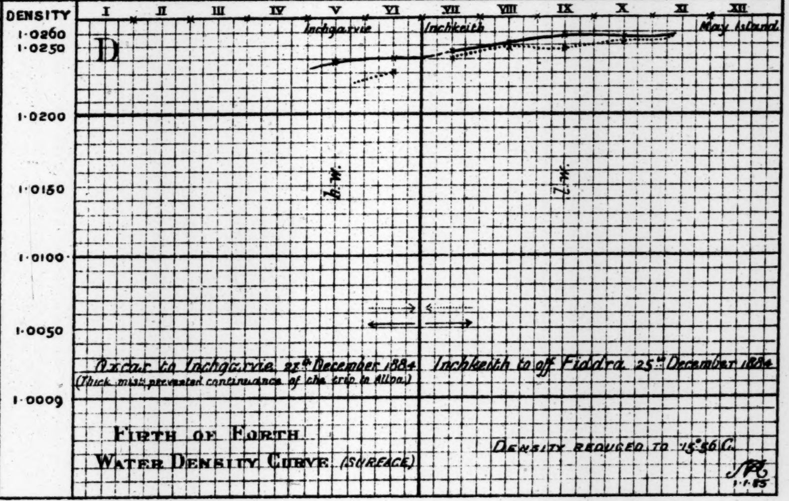
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	DENSITY	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII

Proc. Roy. Soc. Edinr., Vol. XIII, Plate IV.









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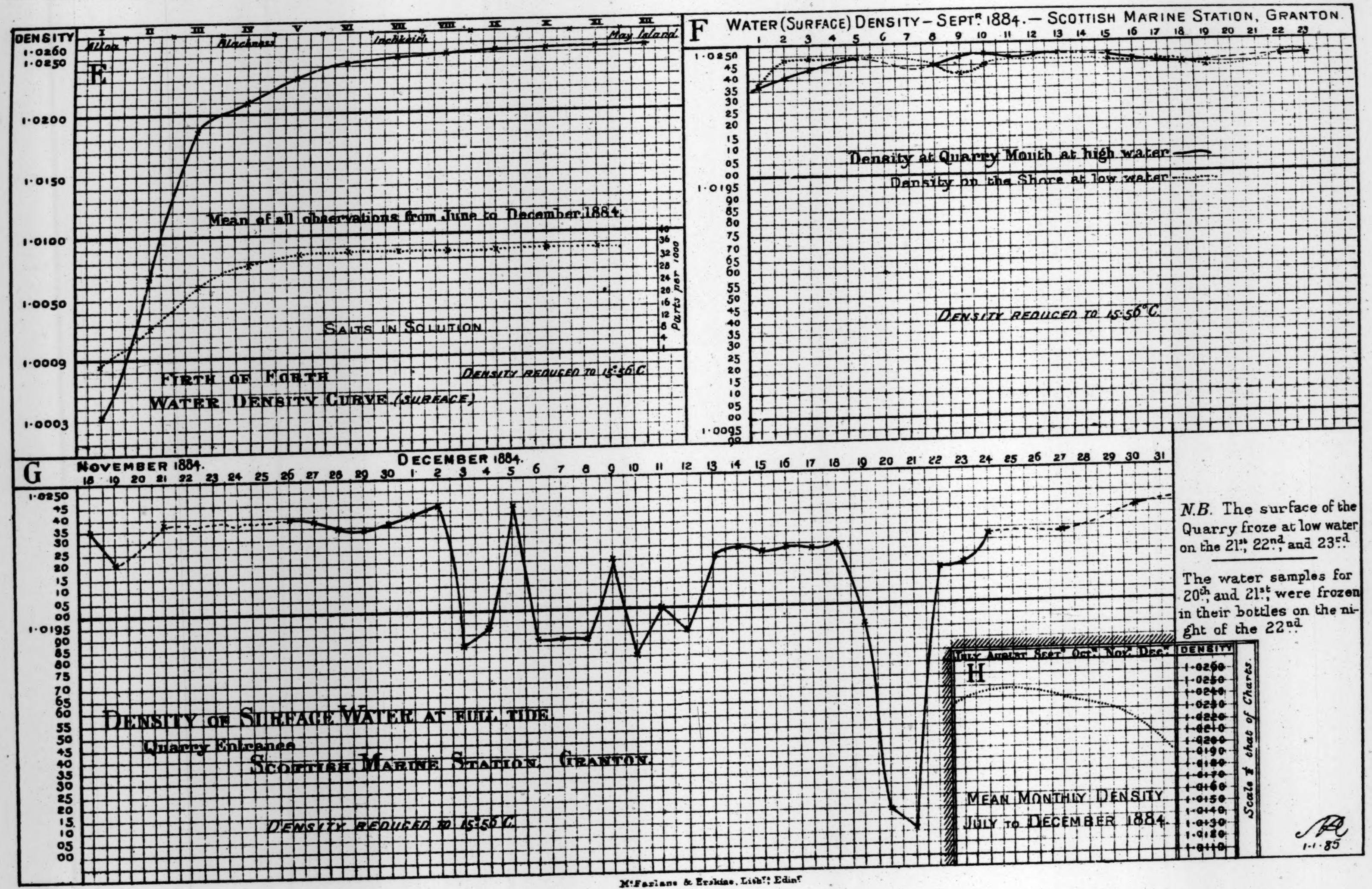
DENSITY I II III IV V VI VII VIII IX X XI XII

WATER (SURFACE) DENSITY. SEPT^{R} 1884. — SCOTTISH MARINE STATION.



N.B. The surface of the Quarry froze at low water on the 21st, 22nd, and 23rd.

The water samples for 20th and 21st were frozen in their bottles on the night of the 22nd.

1·1·85

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stop the vessel more frequently than necessary, and surface samples are usually collected by means of a clean bucket, care being taken to draw it forward of the ejection pipe of the condenser. When brought on board a thermometer is immersed for a minute and the temperature noted. The water is then bottled, tied down, and labelled.

To get water from any desired depth below the surface the oldest method is to use a valved-box arrangement. This method was devised by Hooke more than two centuries ago, and until recently it was the only one employed. When descending, both the upper and lower valves (which open upwards) are kept open, and the water traverses the arrangement. On pulling up, the valves close, and, kept down by the pressure of the water above, they preserve the enclosed sample unmixed. A very ingenious apparatus of this nature, the cistern thermometer of Sir Robert Christison, was presented to the Station; but although convenient in many ways, it has the defect of uncertainty in action, and the result of a long series of trials decided me to employ other methods.

It was tried to collect deep samples by sinking a stoppered bottle to the required depth, and then pulling out the stopper by a special line. The two lines were frequently entangled, owing to currents making the bottle rotate; but although this could have been remedied, in part at least, by fixing the bottle in a frame provided with a vane-shaped attachment, other considerations led to the abandonment of the method. When the stopper fitted closely, the pressure of the superincumbent water prevented its withdrawal by any force less than sufficient to haul the bottle up again in spite of a heavy lead attached to it; and when the stopper did not fit closely, water from intermediate depths found its way in.

The depths in the Firth of Forth are too small (only in one place coming up to 40 fathoms) to permit stopcock water-bottles to be used, and the slip water-bottle was consequently adopted for general employment.

The instrument hitherto used was constructed as an experimental form for Mr Buchanan, and it is about to be superseded by an improved apparatus on the same principle.

Buchanan's slip water-bottle consists of a brass body A (fig. 1), in which a brass disc a' supports three radiating sheets of brass c that are surmounted by a brass dome d, on the top of which there is a ring for the line. The plate a' has an india-rubber ring a fixed upon it, and its under surface has two rings for attaching the

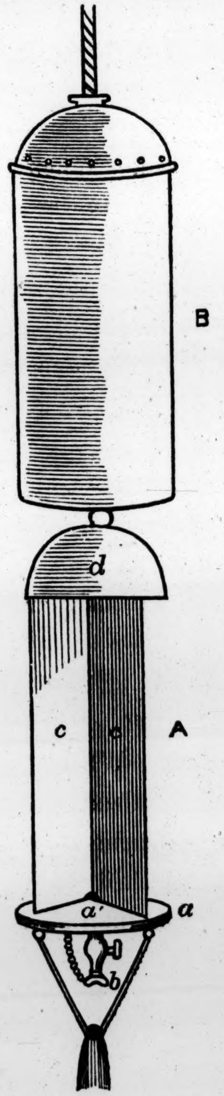

Fig. 1.

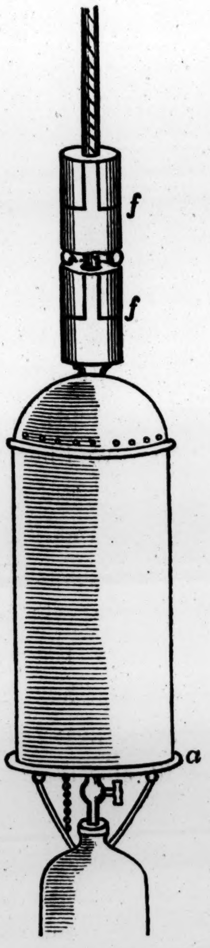

Fig. 2.

lead, and a stopcock b for running off the water. There is also a brass cylinder B, the edge of which rests upon the india-rubber ring a when the instrument is closed (fig. 2).

On board the "Medusa," the steam-yacht of the Marine Station, the water-bottle is attached to the sounding line, which is wound on a drum worked by a small deck engine. The line passes from the drum through leading blocks to the derrick, which is fixed so as to project over the port bow, and when a sample of water is to be obtained from the bottom or an intermediate depth, the water-bottle has a 7-lb. lead attached to it, the stopcock is closed, and a little plug screwed in to prevent the entrance of mud should it strike the bottom. It is then lowered, the slip cylinder B being held in the hand. When the desired depth is reached the slip is let go, it crashes down on the frame, and is guided by the brass strips *c*, *c* on to the india-rubber ring, enclosing a sample of water.

A number of trials showed that when the slip came down it did not press sufficiently strongly on the india-rubber. To remedy this a heavy leaden ring was fixed to the slip, but the result was that the edge struck the dome *d* so heavily that it was indented, and so left a little channel between the rim and the india-rubber. Subsequently it has been the habit, after the slip has struck the body, to let down one or two of Captain Rung's brass weights (*f*, *f*, fig. 2). These rest on the top of the slip cylinder, and press it firmly down. Repeated trial and continuous use for several months have shown this manner of water-collecting to be satisfactory.

When the water-bottle is hove up, the mud-plug is withdrawn, about a quarter of a litre of the water is run out into a bottle which is rinsed out with it, and then filled to overflowing, the stopper inserted, and tied down.

The bottles used are glass-stoppered, blue glass, half-Winchesters, which hold about 1·5 litre. They are packed in boxes, fifteen in each, so as to be carried easily and safely. Each bottle is labelled as it is put aside, with particulars of the date, hour, and temperature.

The temperature below the surface is ascertained by means of the Negretti and Zambra thermometer in the Scottish frame, which was described to this Society in July 1884 (*Proc. Roy. Soc. Edin.*, xii. 928). The frame has been improved by using a small vice arrangement to clamp the line at the upper end, while a cork-screw-shaped attachment at the foot keeps the line alongside the frame.

This means of putting on the thermometer is a great saving of time and trouble. The thermometer has been found to answer admirably.

When each sample is taken the following observations are made and recorded:—

Date.
Hour of collection.
Position by bearings.
Depth of water.*
Depth from which sample was taken.
Temperature of the water at that depth.
Temperature of the air.
Nature of the weather, wind, and state of the sea.
State of tide.
Colour and transparency of the water.*

The colour of the water is observed by sinking a disc of iron, painted white, to the depth of a few feet or fathoms, according to circumstances, and noting its colour. The transparency may be roughly measured by observing the distance to which the disc remains visible.

It is important that the actual notes of all observations be preserved for future reference should uncertainty arise regarding them. There are difficulties in doing this, for it is not easy on a small vessel when there is any sea on to keep an ordinary note-book from getting wet, especially when it is raining; and turning wet pages in a breeze is not easy. I have employed a leather case to hold a note-book, using one page for each set of observations, and keeping it shut when not in use. The principal objection to this is the unwieldy size of the case, and I now propose to use cards, with memoranda of the observations to be made printed on them, and kept in a similar though much smaller case. As each card is used it may be slipped beneath the others, as is done in a date-case. The cards can be very easily kept in boxes, and may be readily and rapidly referred to at any time.

---

* These are sometimes omitted in the case of surface samples.

2. *Determination of the Density.*

The density of the samples of water collected in the firth is determined by means of a very delicate hydrometer of the form used on board the "Challenger."

The hydrometer is made of glass, the tubes for body and stem having been very carefully selected to ensure uniformity of diameter. The instrument has a body of about 5 centimetres diameter, and 12 centimetres long; the stem is nearly the same length, and has a diameter of 3 millimetres. The process of making and calibrating the hydrometer has been described in great detail by Mr Buchanan, in his "Challenger Report on the Specific Gravity of Ocean Water" (*Chall. Rep. Phys. Chem.*, vol. i. pt. ii. pp. 1–4).

The hydrometer which has been used at the Marine Station is provided with seven movable weights, which can be attached to the top of the instrument, and thus adapt it for observing densities of various amount. The first four weights slip over the top of the stem, and rest there, the others are ring-shaped, and are used to supplement the weight of the former. The hydrometer weighs *in vacuo* 150·1478 grammes. The weights have the following values:—

1. 4·0285 2. 3·2247 3. 2·4099 4. 0·8195
5. 0·8125 6. 0·5184 7. 0·3312

As each of the first four weights can be used with any or all of the remaining three, and as, with a little trouble, any of the last three can be balanced on the top of the stem by itself, the total weight of the hydrometer may be varied from 150·1478 to 155·8384 through 36 gradations.

The volume of the body and bulb of the instrument is 150·2070 cubic centimetres at 0° C. At 25° C. it is 150·3210, the volume of the 100 millimetres into which the stem is divided is 0·85 c.c.; and as it is assumed to be uniform, the volume of each millimetre of the stem is taken as 0·0085.

Most of the density observations were made on the "Ark," the floating laboratory of the Station. As she is perfectly steady, except when there is a conjunction of full tide and high wind, it was not necessary to use a swinging table. The process of making an observation of the density is as follows:—

A tall glass jar, capable of holding a litre, is rinsed out with the water the density of which is to be determined and which has the temperature of the laboratory, and then filled up to the 800 c.c. mark. A thermometer, the error of which is exactly known, is hung in the water, read after being immersed for 1 minute, removed and the hydrometer put in, sunk up to the top of the scale, 1 centimetre from the top of the stem, and allowed to rise. A weight, sufficient to sink the instrument until not more than one-third of the stem is submerged, is then put on, and after it has come to rest, the scale is read to half a millimetre. Another weight is added, so that not less than two-thirds of the stem are immersed; after coming to rest the readings are taken, the weights removed, the hydrometer dried and replaced in its box, and the thermometer again hung in the liquid for a minute. The temperature being noted, the thermometer is withdrawn, and the water poured back into the bottle. The whole operation occupies about ten minutes, and gives data for two distinct values of the density. The hydrometer is not read closer than to half a division, although with care it might be taken to one-third.

A table giving the volume of the hydrometer at every tenth of a degree centigrade, from 0° to 25°, has been drawn up, and from this table the volume of the body at the observed temperature is taken; the volume of the stem immersed is got from a table which gives the value for each half-millimetre from 0 to 100. These added together give the immersed volume, and the weight being taken from another table and divided by this volume gives the density *at the observed temperature.* The mean of the two densities is taken, and reduced from the mean of the two corrected observed temperatures to 15°·56 C. by means of Dittmar's table (*Chall. Rep. Phys. Chem.*, vol. i. pt. i. p. 70).

The Specific Gravity Observation Book contains a copy of the observations (see p. 34) on the upper half of each page, and below it the following particulars:—

Date and hour of determination.

Temperature of Water (t'_1) and its correction.

1*st Weight on hydrometer.*

Hydrometer Reading.

2*nd Weight on hydrometer.*

Hydrometer Reading.
Temperature of Water (t'_2) and its correction.
Density at t'_1 (1).
Density at t'_2 (2).
Mean density (of 1 and 2) at t' (mean of t'_1 and t'_2).
DENSITY at 15°·56 (specific gravity, water at 4° being unity).
Calculated value of CHLORINE (χ)
Permilleage of CHLORINE found (χ').
Alkalinity.

The numbers of the first six entries are put down in the book as read off the instruments, the others are copied from the calculation book.

In order to get some idea of the accuracy of the method—that is of the exactness of the readings, the correctness of the tables, and the accuracy of the calculations—the following determinations of the density of distilled water were made. The temperature was varied by the addition of hot distilled water.

I. Temperature varied from 4°·5 to 4°·6.

Weight 4.	Weight 6.	Weight 7.	Mean.
1·000022	1·000017	1·000014	1·0000175
		Calculated,	0·999997
		Difference,	0·000020

II. Temperature varied from 4°·6 to 4°·9.

Weight 4.	Weight 7.	Mean.
1·000019	1·000010	1·0000145
	Calculated,	0·999995
	Difference,	0·000019

III. Temperature varied from 14°·9 to 14°·2.

No Weight.	Weight 4.	Weight 7.	Mean.
0·999154	0·999331	0·999313	0·999299
		Calculated,	0·999253
		Difference,	0·000046

IV. Temperature varied from 11°·9 to 11°·4.

Weight 4.	Weight 7.	Mean.
0·999725	0·999731	0·999728
	Calculated,	0·999538
	Difference,	0·000190

Mean of four determinations,	0·9997648
Mean of calculated densities,	0·9996982
Difference,	0·0000666

Omitting the exceptionally high discrepancy of IV. the difference as a mean of three results is 0·000028, and even including it, it does not touch the fourth place.

To form an idea of the reliance to be placed on the hydrometer readings, all those cases (164 in number) in which the weights 1 and 1 + 7 were used in consecutive determinations of one water, were investigated and classified. Temperature does not appear to affect the differences.

Let r be the reading with weight 1, and r' with weights 1 and 7 in the same water, then, as these weights can only be used for water of nearly the same density, in a perfect hydrometer perfectly read $r-r'$ should be a constant, and in an actual instrument read with ordinary care the mean of a sufficient number of observations should approximate very closely to the constant. In 164 cases $r-r'$ had the following values:—

Values,	38	37·5	37	36·5	36	35·5
Times,	2	16	48	49	40	9

The mean of all the values is 36·5. Hence out of 164 cases—

49,	or 29·9	per cent.,	differed from	the mean by	0
88,	or 53·6	”	”	”	0·5
25,	or 15·3	”	”	”	1·0
2,	or 1·2	”	”	”	1·5

Here the maximum variation from the mean is 1·5, while nearly 84 per cent. of the observations showed a deviation no greater than 0·5.

0·5 of a division of the hydrometer corresponds to a difference in density of 0·00003 in the case of a sample of sea water of ordinary density, while 1·5 division is equivalent to a difference of 0·00009, the temperature being the same.

The four triple determinations of distilled water give the total error as 0·000066 as a mean of the whole, and in all cases the variations were in the same direction.

As stated in describing the method, each determination of density was made in duplicate, and the results independently worked out. The two densities obtained in this way invariably differed, that obtained when the greater part of the stem was immersed being always the greater. The differences between the two readings in 204 cases have been classified and examined.

The following table (I.) exhibits the results, which are not apparently affected by temperature, deviations of each magnitude appearing in densities determined at all temperatures from 4° to 20°. The differences are given in units of the 5th place.

TABLE I.—*Discrepancies in Double Determinations.*

Discrepancy, $\delta - \delta'$.	∠3	3	4	5	6	7	8	9	10	11	12	>12
Number of cases in 204, . . .	1·3	16	4	19	32	7	25	19	4	16	19	29
Percentage, . .	6·3	7·8	1·9	9·0	15·6	3·4	12·2	9·0	1·9	7·8	9·0	14·2
Percentage of cases where one reading was greater than 80, . .	23	37·5	75	42	25	43	76	58	50	68·7	73·7	73

The mean of all these discordances is 8·4, but omitting the 14·2 per cent. over 12, as being probably due to careless readings, the mean becomes 6·9. In order to take the least favourable view of the case, the higher number will be considered, and as the mean of the two observations was always taken for the correct density, this discordance is reduced to 4·2 as deviation from the mean.

It is not allowable to change this 4·2 as *error*, since it is always positive, and since the amount of it is roughly proportional to the difference between the length of stem immersed by the addition of the second weight, and more nearly proportional to the number of the reading. The last horizontal line of the preceding table shows that (omitting $\delta - \delta' = 4$, and $= 10$ on account of the small number of cases) the percentage of readings over 80 increases, roughly speaking, as the discrepancy increases.

The reason of the difference may be that the stem of the hydrometer is slightly conical, instead of being truly cylindrical, as assumed. It might also be that the weight of the brass pieces was not correctly known, but a careful reweighing showed that this was not so. Capillarity should act equally in each case, and need not be considered. The only remaining explanation which suggests itself is the difference in weight produced by the film of water adhering to the part of the stem above the surface. To test this the density of three samples of water was determined twice with the hydrometer stem dry, and twice immediately after the the stem had been immersed. The results are shown in Table II.

Temperature unaltered, about 7°.

Table II.

Weights.	Water Sample No. 263, Hydrometer stem.			No. 267, Hydrometer stem.			No. 268, Hydrometer stem.		
	Dry.	Wet.	Dif.	Dry.	Wet.	Dif.	Dry.	Wet.	Dif.
1	86·5	84·0	2·5	95·5	94·0	1·5	99·0	96·5	2·5
1+7	49·0	48·0	1·0	...	...	...	62·0	60·5	1·5
1+6	...	...	...	37·5	37·0	0·5	...	...	...

Here the adhering film of water produced a sinking of the hydrometer of from 0·5 to 2·5 millimetres, according to the length of exposed stem. This is an error which is practically negligible when the reading is less than 30, but rises to a serious amount when it is greater than 80.

Taking 2·0 mm. as the amount of sinking produced by the film of water when the stem reading is over 80, and since the sinking produced by weight No. 7 (0·3312 grammes) is on an average 36·5 (see page 38) the weight of the water is $\frac{0{\cdot}3312 \times 2}{36{\cdot}5}$ *i.e.*, $= 0{\cdot}01814$. In water sample No. 250(1) the reading was 80 and the calculated density 1·02734. Supposing the scale to have been wet, and adding 0·01814 grms. to the weight of the hydrometer, the density comes out as 1·02746, an increase of 0·00012.

Here there is a sufficient explanation of the discrepancy increasing with the value of the reading; and since in each case the mean of the determination when the stem was almost all out of the water, and that when the stem was almost entirely immersed, and when consequently the error from the attached film was reduced to almost nothing, the uncertainty of a determination (from this cause) is reduced by a half, and comes to be 0·00006, exactly 0·00002 more than that brought out by the discussion of the 204 discrepancies without consideration of their cause. The reason of this, assuming the above explanation to be correct, and considering that 0·00006 is the value of an extreme case, reading over 80 and weight 7, is obvious; as generally the reading is under 80, and when over it weight 6 is usually employed as an addition which immerses more of the stem. Had the scale been always used when dry greater accuracy could be expected; but, on the other hand, it must not be supposed that the weight of the film of water was always equal to 0·01814 grm., for often the reading was not taken until the greater part of the water carried up had run down again.

The occurrence of the maximum deviation from the correct reading (1·5 mm.) in the same direction as the increase due to the water film in an extreme case would certainly affect the fourth place to the extent of 1, but the probability of such an event happening is very small, and the sum and difference of the effects of the error of reading and the mean error due to the water film are respectively 0·00007 and 0·00001.

The result of the discussion is that the discrepancies between the two observed densities of one sample of water are accounted for, perhaps not fully, but satisfactorily, in so much that it is beyond question that the hydrometer gives results which for the purposes of comparison are rigorously correct to the fourth decimal place. It also shows that by taking further precautions greater accuracy may be reasonably looked for.

3. *Determination of Alkalinity.*

Sea water has an alkaline reaction which is supposed to be due to the presence in solution of carbonates, and especially of carbonate of lime. The measurement of the alkalinity is then a measurement of the amount of dissolved carbonate.

It is determined by Tornøe's method, boiling with dilute hydrochloric acid and titrating the excess with potash, using aurine as an indicator.

Practically 250 c.c. of the water are measured in a flask, poured into a porcelain basin, a few drops of neutral aurine solution added, 15 c.c. of standard hydrochloric acid run in from a burette, and the whole heated over a large spirit-lamp for from twenty to twenty-five minutes. It usually boils for from ten to fifteen minutes. The excess of free acid is then determined by means of potash, the red colour of alkaline aurine being dispelled by a few drops of acid, and restored by potash until the end point is closely defined. Each burette reading is taken down on a card, and the result copied into the Laboratory Book. The cards are preserved for future reference if necessary.

The standard acid and alkali were at first made up to be of the strength of $\frac{1}{22}$ normal, *i.e.*, $\frac{1}{22} \times 36{\cdot}5$ grms. of hydrochloric acid, and $\frac{1}{22} \times 56$ grms. of potassium hydrate per litre, so that 1 c.c. might correspond to 1 milligram of carbonic acid.

It was, however, found to be more economical of time to make up an acid roughly approximating to the desired strength, and to determine its chlorine very carefully by titration with silver nitrate. The potash was made equivalent to the acid.

Two solutions of hydrochloric acid have been used for the work described here. The first was found by the mean of five concordant experiments to have 1 c.c. equivalent to 1·007 c.c. of the potash used. More than 5 c.c. of potash is never used, except in very exceptional cases, and as the burette is not read closer than 0·05 c.c. the difference between 5·00 and 5·03 is considered negligible. The mean of the experiments with the second acid solution gave 1 c.c. of acid as equivalent to 0·990 c.c. of alkali, and this also is taken as equality. The error of considering the two acids as equal is only introduced in the titration of the excess, the quantities of acid used being reduced separately according to the amount of chlorine found in them by analysis.

Trustworthiness of the Method.—A large number of experiments were made on the same samples of water, both for the sake of practice and in order to arrive at the probable uncertainty of each experiment.

The following tables (III. and IV.) give particulars of these determinations, and of the corresponding deviations from the mean.

If the five cases out of the thirty-five in which the deviation exceeds 0·5 be omitted, as probably due to mistakes or misreadings, the mean deviation is reduced to 0·24.

It is probable that 0·2 may be nearer the truth, as it was never the practice to filter the water before determining the alkalinity, and after the bottle had stood in the laboratory for a day or two, the 250 c.c. decanted off was tolerably free from suspended particles; while, when four or five determinations were made on water contained in a bottle, each quarter litre would contain more sediment than the one before, and a minute shell makes a distinct increase in the alkalinity. Looking at the eight cases epitomised in the preceding table (III.), where three or more determinations were made on one water, it is evident that in four (*a*, *c*, *e*, and *k*), the later determinations show an increased alkalinity.

TABLE III.—*Alkalinity Determinations.*

No.	Sample.	I.	II.	III.	IV.	V.	Mean.
a	68	11·75	12·0	12·1	...	...	11·95
b	69	11·5	11·1	10·25	11·5	...	11·09
c	70	14·05	13·9	14·65	...	...	14·21
d	71	11·9	12·15	11·75	...	...	11·93
e	72	11·0	11·55	11·9	...	...	11·48
f	73	11·65	11·55	...	...	...	11·61
g	74	12·1	11·75	...	...	...	11·92
h	92	12·8	12·4	11·7	12·4	...	12·31
i	93	12·2	11·8	...	...	...	12·01
j	261	9·2	10·65	9·9	10·65	9·8	10·04
k	262	10·05	10·1	9·85	11·0	...	10·25

TABLE IV.—*Deviations from Means.*

	a.	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*	*i.*	*j.*	*k.*
I.	·2	·4	·15	·04	·48	·05	·18	·5	·2	·84	·2
II.	·05	·0	·3	·22	·07	·05	·18	·1	·2	·61	·15
III.	·15	·8	·45	·18	·41	...	...	·5	...	·14	·4
IV.	...	·4	...	...	...	...	...	·1	...	·61	·75
V.	...	...	...	...	...	...	...	...	...	·15	...

Mean deviation 0·29.

The quantity represented by an alkalinity is very small, although the number used to express it is large. An alkalinity of 50, means that in a litre, say 1026 grammes, there is 0·05 gramme of carbonic acid as calcium carbonate; that is, a percentage of 0·00487; which, taking the worst view of the inaccuracy of the determinations, might vary from 0·00498 to 0·00476.

In the discussion of the alkalinity of the Firth of Forth in this paper, the integral figures may be viewed as correct, the decimal part being uncertain.

It would impart a greatly increased certainty to the figures if the water had always been filtered before the alkalinity was determined, and if duplicate analyses were invariably made and the rigorous zigzag system of titration adopted. This will be done in future.

A great many determinations of the chlorine, or rather total halogen, present in the samples of sea water were made, but, from various causes, the probable error of each experiment was so large as to render the results useless for the purpose of detecting the difference between the ratio of salinity to density in estuary water and in ocean water. For this reason the figures are not published.

The density of the water may be taken as an index of its salinity, although there are no data available for interpreting each density into its corresponding percentage of total salts. This can only be done in the case of ocean water proper, for which tables have been constructed by several chemists.*

II. Results of Preliminary Work.

1. *Notes of Previous Work.*

In 1816 Dr John Murray read a paper to this Society "On the Composition of Sea Water," the samples which he analysed being taken from the Firth of Forth near Leith. The paper (*Trans. Roy. Soc. Edin.* for 1816) contains results of great theoretical value, which were instrumental in modifying the theory of the existence of salts of different bases and acids in solution, and which altogether changed the mode of analysis of sea and mineral waters. Attention was given more particularly to the solid constituents, and no observations seem to have been made by Dr Murray on the variations in salinity at different parts of the firth.

Dr John Davy published a paper† in 1843, "On the Temperature and Specific Gravity of the Water of the Firth of Forth." He examined the temperature and density of the water at the end of Leith Pier on eight occasions, at intervals of about a month. His entire observations are reproduced in table (V.).

It was Davy's intention to continue the monthly observations for a number of years, but he had to leave Edinburgh, and they were stopped. Since no particulars as to how the densities were determined were given, it is impossible to compare them with others at a later date.

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* See Dittmar, *Phys. Chem. Chall. Rep.*, I. pt. i. 1884.

† *Ed. New Phil. Journ.*, xxxvi. p. 1.

Table V.—*Davy's Specific Gravity Observations.*

1842.	Temperature.		Density.	Tide.	Wind.
	Air.	Sea.			
Sept. 30, 3 0 P.M.	55°	54°	1·0258	Low water.	N.E.
Oct. 29, 2 0 ,,	43	45	251	½ ebb.	N.
Nov. 30, 3 0 ,,	45	42	256	,,	W.
1843.					
Jan. 5, 3 0 ,,	39	42	248	⅔ flood.	N.
Feb. 5, 3 30 ,,	35	38	197	½ flood.	N.
Feb. 19, 3 30 ,,	39	38	242	,,	S.E.
March 4, 2 0 ,,	39	38	205	,,	N.W.
April 22, 2 0 ,,	55	44	248	,,	W.

Dr Stevenson Macadam investigated the salinity of the Firth of Clyde in 1855.* He observed the specific gravity at more than fifty places, and determined the total solids and chlorine in each. He found that between Bowling and Renfrew there was a change produced by the tide between high and low water equal to that discerned in a distance of five miles along the firth at low water.

In subsequent investigations he examined the Firths of Cromarty and Inverness. The results are recorded in the *Proceedings* of this Society for 1866.†

Professor Kyle of Buenos Ayres made some observations in 1874 on the River Plate, in the same way as Dr Macadam on the Clyde. I am indebted to Mr F. Newman for a translation of Kyle's Spanish pamphlet (*Algunos datos sobre la composicion de las aguas del Rio de la Plata*), and for a chart of the Plate, with the water sampling stations.

The results brought out by Professor Kyle are interesting, but like the other observers cited above, he neglects to mention whether his specific gravities are reduced to 0°, to 4°, or to 15°·56, or whether water at 0°, 4°, or 15°·56 was taken as unity. It is therefore impossible to consider the results except as purely relative to the estuary in question, and no comparison between the different investigators can be made.

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* *Brit. Assoc. Reports*, 1855, ii. 64.

† *Proc. Roy. Soc. Edin.*, V. (1866), p. 370.

The Cattegat, Skager Rack, Baltic, and north-eastern parts of the North Sea, have been made the subject of very careful and prolonged examination by various Danish and German scientific workers. Water samples have been taken regularly for a number of years at various points along the coasts, and from lighthouses and lightships at considerable distances from land. The results of the examination of these samples, from 1872 to 1881, is tabulated in conjunction with the meteorological conditions, especially with respect to rainfall, in a recently issued paper by the Commission in Kiel for the Scientific Investigation of the German Seas.*

The general low densities of these waters, and the variations to which they are subject, make the conditions which obtain there not unlike those in an estuary, but they are in general more complicated and difficult of investigation.

While it is fully realised that it will take years of consecutive observations to thoroughly settle the relations of the fresh and salt water in an estuary; and that many conditions, such as the currents, law of the tides, and rainfall over the area drained by the principal river and its tributaries, must be taken into account, it is considered expedient to state the results observed in the six months, from June to December 1884, on the Firth of Forth. These results are purely preliminary, but as little attention has been given to such matters hitherto, they may prove of interest, and may lead to suggestions for improvements in the methods of carrying on the work.

2. *The Firth of Forth.*

The River Forth rises in the valley between Ben Lomond and Ben Venue, is joined near Stirling by the Teith, and gradually merges into the Firth of Forth, the precise point at which the river ends and the firth begins being a matter permitting of difference of opinion. Probably the best plan is to view the river as ending at Queensferry, but for convenience the term Firth of Forth may be applied as describing the river and firth proper from Alloa to the Isle of May, a distance of 55 miles. According to Keith Johnston, the area drained by the Forth is 500 square miles. Few large rivers flow into the firth, those of any importance are, on the north side the *Black Devon* at Clackmannan, and the *Leven* at Leven; on the south side there are the *Carron* at Grangemouth, the *Avon* a few miles further east, the *Almond* at Cramond, the *Water of Leith* at Leith, the *Esk* at Musselburgh, and the *Tyne* near Dunbar. All these, together with the numerous small burns, are represented on the chart (Plate III.), which also shows the depth of the water in the firth.

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* *Vierter Bericht für die Jahre* 1877 *bis* 1881, Berlin 1884,—Periodische Schwankungen des Salzgehaltes im Oberflächenwasser in der Ostsee und Nordsee, von Dr H. E. Meyer.

From Alloa the depth of the water is under 10 fathoms until within three miles of Queensferry, where it increases at first gradually, then, at the Bamer Beacon, abruptly to over 30 fathoms, and close to Inchgarvie to over 40. This is the deepest part of the firth, and the narrowest. A very strong tide runs in the channels on each side of Inchgarvie. The deep water is confined to a small area, and the 10 fathom stream runs along the northern shore until off Kirkcaldy, where it widens out in a funnel shape and approaches the coast on each side. There is a short tract over 10 fathoms to the south of Inchkeith known as the Narrow Deep. Several small depressions of more than 20 fathoms occur between Queensferry and Inchkeith, and a little to the east of that island the 20 fathom area begins as a narrow stream trending northward and spreading out off Largo. The Isle of May is connected to the mainland of Fife by a submerged plateau rising to less than 20 fathoms from the surface, and about 4 miles east of the May depths beyond 30 fathoms commence.

A line drawn from Aberlady Bay to Largo divides the firth into two very different halves. To the west of it, the slope of the bed is extremely gradual and the depth slight; to the east of it, the shore slopes down abruptly, and the bed of the firth is, with one or two insignificant exceptions, uniformly over 20 fathoms in depth.

3. *Observations on the Surface Salinity in the Firth.*

It is assumed that the amount of total salts may be deduced from the density as if estuary water were ocean water, diluted with pure water. The assumption is that the percentage composition of the total salts is the same as regards their constituents, whether the water yielding them contain 1 or 4 per cent. The constancy of composition for ocean water was shown in 1865 by Forchhammer to be highly probable, and the researches of Tornøe, Schmelk, and other chemists of the Norwegian North Atlantic Expedition, crowned by Professor Dittmar's exhaustive discussion of the "Challenger" waters, have established it as a definite certainty.

This cannot be the case with estuary water, as the salts carried down by rivers are in quite different proportion to those found in the sea, and before the processes occurring there have had time to produce uniformity of composition; that is, where river water predominates, the proportion of the salts among themselves must vary. Consequently, until exact experiments can be made on this point, the interpretation of estuary densities by ocean-water tables must be taken with reservation.

To get a preliminary view of the rate of freshening in the Firth of Forth, it was determined in September 1884 to make a monthly trip for collecting water samples from the entire firth; and on the 18th of September the "Medusa" proceeded from Inchkeith to Grangemouth for that purpose. Surface samples were taken every five miles (the positions are marked on the chart), and bottom samples at each alternate station. Observations were made both in going and in returning, and they are represented on the small diagram A (Plate IV.), the former as a dark, the latter as a light line.

The tide was ¾ flood at Inchkeith; at Grangemouth, where we turned, it was two hours ebb; and on the return to Inchkeith was ¾ ebb. The weather was fine, but hazy throughout.

The intention to make the complete tour of the firth in one day had to be reluctantly relinquished, and the Inchkeith to May section was completed on the 25th. Inchkeith was reached at 10.30 A.M. with ¾ ebb tide, and a strong south-westerly breeze. Surface samples were taken at the Stations marked on the chart VIII., IX., X., and XI. At the May the tide was ¾ flood, and a south-westerly breeze rising to a gale, which increased towards evening, prevented water samples being obtained at Stations IX., VIII., and VII. returning. This trip showed that the density of the water samples decreased steadily, gradually, and uniformly from the May to the Oxcar Beacon (V.), but that the change then became more rapid, the curve resembling a portion of a rectangular hyperbola. The weather for the previous few weeks had been dry and warm.

On October 7, water samples were taken to and from the May from 8 A.M. to 7 P.M. The weather was fine but dull, and the sea smooth. It was low water at Station VIII. going, and high water at Station XI. returning.

Next day the "Medusa" went to Alloa, in rain, slight mist, and a north-easterly breeze. Water samples were taken as before. It was low water at Inchgarvie (Station V.) going up, and high water at the same place returning.

The water, beautifully clear and transparent, and of a deep green-blue colour at the May, became light green and less transparent about Inchkeith, and from Inchgarvie onwards it was yellow and very muddy.

The curve for this trip (Plate IV., B) is less regular, but of the same general shape as the previous one.

The November trips were made on the 10th and 11th of the month. For the previous week or two there had been high wind and much rain, and the rivers had been flooded heavily, but were then subsiding. The 10th, when the eastern half of the firth was traversed, was bright, clear, and calm. The shortness of daylight prevented the yacht from getting further than Station X., at which point it was low water. On the 11th it was dull, with a stiff south-easterly breeze. It was high water at Station VI. going up, and low water at Station V. coming down. The river was greatly flooded, and the curve (Plate IV., C) is irregular, and much more abrupt than the others. The reading of the density at Station IV. going up seems too low, but there is no reason to suspect its accuracy. The influence of the flooded streams may be traced by a lowering of density down to Station IX. The effect of this "spate" was to reduce the density at Inchgarvie from 1·02382 to 1·02029, that at the Oxcar Beacon from the mean of 1·02438 to 1·02022, that at

Inchkeith from 1·02472 to 1·02403, and those at Stations VIII. and IX. from 1·02505 and 1·02518 to 1·02458 and 1·02508 respectively.

The December trip did not take place till the 25th, when my friend Mr T. Morton Ritchie was good enough to take charge of the eastern excursion. The day was fine, with a north-easterly breeze, and a slight swell. The tide was two hours ebb at Station VII. going, and one hour flood at the same place returning. On the 27th the yacht started for Alloa, but the morning, which was hazy, gave place to a day of fog, and it was impossible to proceed beyond Inchgarvie. The 29th and 30th were also misty, and this portion of the trip had, very reluctantly, to be dispensed with.

The curve D (Plate IV.) shows that the water had returned to its former value after the diluting effect of the floods of November.

The effect of the tide obscures the changes of salinity to a certain extent in these monthly cruises, but although the data are so few they are sufficient to show that between Inchkeith and the Isle of May the tidal effect is relatively slight, and the variations in density very gradual, though perceptible; while from Inchkeith to Alloa the tidal effect increases with every mile, and the rate of change becomes more and more rapid. Tables VI. and VII. give the figures observed in these consecutive trips.

In order to make some attempt at neutralising the influence of tides, the mean was taken of all the observations made since June at each of the Stations. These were made at all states of the tide, and in all weathers that the yacht could stand, and although they are not very numerous, and do not extend over more than seven months, they give some idea of the alteration in salinity with position.

The means are embodied in the diagram (Plate V., E), on which the calculated amount of total salts is also represented. It plainly shows that the parts of the firth to which least attention has, as yet, been given, those west of Inchkeith, are the most deserving of study, for there the changes are large and rapid.

The maximum, minimum, and mean density observed at each Station, together with the number of cases which give the mean, are tabulated. (Table VIII.)

TABLE VI.—*Density of the Water at 15°·56.*

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.	XII.
September 1884, .	...	...	1·02082	1·02285	1·02444	1·02470	1·02499	1·02533	1·02531	1·02537	1·02549	1·02555
October 1884, . .	1·00160	1·01088	1·01911	1·02272	1·02332	1·02443	1·02505	1·02512	1·02547	1·02525	1·02555	1·02558
November 1884, .	0·99923	1·00272	1·01419	1·01704	1·01946	1·02022	1·02403	1·02458	1·02501	1·02529	...	...
December 1884, .	...	...	...	...	1·02351	1·02353	1·02464	1·02511	1·02508	1·02545	1·02554	...

TABLE VII.—*Alkalinity.*

September, . .	...	...	...	...	49·1	50·4	49·6	...	...	...	...	...
October, . . .	...	...	...	...	...	...	...	...	...	...	...	..
November, . .	1·2	11·6	31·4	39·2	44·3	45·2	48·3	48·6	50·5	49·7	...	...
December, . .	...	...	...	...	48·6	49·6	51·2	52·3	51·4	51·7	53·9	..

The mean of the density, &c., at the Stations going and returning is given here.

TABLE VIII.—*Variations in Density.*

Station.	Maximum.	Minimum.	Mean.	No. of Obs.
I. Alloa,	1·00160	0·99923	1·00042	4
II. Kincardine,	1·01521	1·00070	1·00680	4
III. Hen and Chickens Buoy,	1·02169	1·01084	1·01898	11
IV. Blackness,	1·02414	1·01650	1·02087	6
V. Inchgarvie,	1·02461	1·01863	1·02303*	11
VI. Oxcar,	1·02492	1·01953	1·02355†	10
VII. Inchkeith,	1·02526	1·02380	1·02472	13
VIII.	1·02544	1·02450	1·02505	7
IX.	1·02551	1·02495	1·02518	8
X. Off Fiddra,	1·02557	1·02417	1·02530	9
XI. Off Bass Rock,	1·02566	1·02526	1·02547	6
XII May Island,	1·02570	1·02443	1·02511	5

Differences between Mean Densities in units of the fifth place.

I. II. III. IV. V. VI. VII. VIII. IX. X. XI. XII.

638, 1218, 189, 216, 52, 113, 33, 13, 12, 17 – 36.

Note.—All the densities given are at 15°·56 C.

An examination of Table VIII. shows that for maximum and minimum, as well as for mean observed densities, the increase is perfectly continuous until the last Station is reached, when the maximum density is only probably greater than that at Station XI., the minimum is certainly less than that at Station VIII., and the mean is less than that at Station IX. It is not easy to find an explanation of this fact, for there is no river nearer than the Tyne at Dunbar, and there are no springs on the Isle of May, the lighthouse keeper being compelled to get his fresh water all carried from Crail. It is, of course, possible that there may be some submarine springs near the island, but no indication of this is given by an examination of the bottom waters, which were always salter than the surface. It is also a curious fact that four samples of water from the firth beyond the Isle of May had a mean density of 1·02514, scarcely greater than that at Station XII., and distinctly less than that at Station IX. The samples at the Isle of May were all collected, as it happens, during flood tide; so that no current of the ebb carrying fresh water can be called in to account for the observation. It would almost seem as if the densities hinted at a maximum about Station IX. with lower densities on each side; if the water of the Firth of Tay is fresher than that of the Firth of Forth a ready explanation suggests itself.

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* Omitting the day of high flood in the river mean = 1·02382.

† „ „ „ = 1·02438.

In consequence of the way in which water-sampling trips were conducted, there were some Stations where the two samples were drawn at opposite phases of the tide. Inchkeith and Inchgarvie were usually in that position. Table IX. shows what difference was observed in the densities at different phases of the tides on the few occasions when these were sufficiently separated. It is of course very fragmentary, as the variations in the difference between high and low water must be considerable, but it is sufficient to indicate the general rule which prevails. The difference is by far the greatest in the upper reaches of the firth, and decreases more

TABLE IX.—*Variation of Salinity with Tides.*

Station.	Date.	Density of Water.			Alkalinity.			Tides.	
		1	2	Dif.	1	2	Dif.	1	2
II.	8–10–84	1·01521	1·00655	866	...	...	...	3½ hrs. flood	2¼ hrs. flood
,,	11–11–84*	1·00474	1·00070	404	11·1	12·1	−1·0	2 hrs. ebb	4½ hrs. ebb
IV.	8–10–84	1·02414	1·02131	283	...	...	...	¾ flood	¼ flood
V.	18–9–84	1·02461	1·02425	36	50·4	47·9	2·5	High water	½ ebb
,,	8–10–84	1·02424	1·02241	183	...	...	...	,,	Low water
,,	11–11–84*	1·02029	1·01863	166	46·5	42·1	4·4	,,	,,
VII.	18–9–84	1·02505	1·02481	24	51·1	48·2	2·9	¾ flood	¾ ebb
,,	10–11–84*	1·02427	1·02380	47	47·5	50·1	−2·6	2½ hrs. ebb	2½ hrs. flood
IX.	25–9–84	1·02522	1·02539	−17	...	...	...	1 hr. ebb	Low water
,,	10–11–84*	1·02508	1·02495	13	51·3	49·7	1·6	3¾ hrs. ebb	¾ hrs. flood
XI.	7–10–84	1·02544	1·02566	−22	...	...	...	High water	2 hrs. flood

* River in flood.

STATIONS.—II. Kincardine; IV. Blackness; V. Inchgarvie; VII. Inchkeith.

and more gradually. Beyond Inchkeith the difference is little noticed, in fact the water is found to be sometimes denser at a lower state of tide. This effect may be due to currents, which were not taken account of in this preliminary investigation. The rate of change of density with the tide decreases very rapidly at first. At Kincardine, the difference touches the second place of decimals; calling the density of pure water 10,000, it is 100, at Inchgarvie it is about 15, at Inchkeith 4, and beyond Inchkeith about 1; that is, 1 in the fourth decimal place by ordinary notation.

The temperature observations made on the monthly trips have not been alluded to here; they are intended to form a separate paper. It was found, speaking generally, that in summer the temperature of the surface water was highest at Alloa, and steadily fell as the Isle of May was approached; the rate of fall becoming more and more gradual. In winter this state of affairs is inverted. The water at Alloa is the coldest, and the temperature rises more and more gradually towards the May. The difference also is much greater between the summer and winter temperatures at Station I. than at Station XII. For example, at Queensferry (Station V.), on July 31, the surface temperature was 58°·2 Fahr., on December 27 it was 41°·2 Fahr., a range of 17° Fahr. At the May, on June 11, the surface temperature was 50°·1 Fahr., and on December 25, within six miles of the May, it was 43°·8, a range of 6°·3.

The change of temperature produces a corresponding change in the density of the water *in situ*, which has important bearings on convection currents, and which must also influence the rate of mixture of sea and river water, especially as the temperature of the river water is, in winter, usually below that of its maximum density.

4. *Surface Water Density at the Marine Station.*

The Scottish Marine Station has a floating laboratory moored in the centre of the submerged Granton Quarry. The entrance is on the western side, and at high water it is continuous with the firth for a considerable breadth. At low water the channel is dry, and in front of it is a broad tract of sand extending out to Cramond Island, The River Almond flows into the firth just to the west of Cramond island, at low water. It is about two miles distant from the quarry.

A sample of water was taken at the quarry mouth at high water daily, with such interruptions, sometimes of considerable duration, as were to be expected at the first starting of a piece of routine work not absolutely necessary, with a small staff.

In July only three samples were taken, owing to a trip of the "Medusa" on the Clyde; their densities varied from 1·02454 to 1·02469.

In August there were 21 occasions on which water was collected at the entrance, 13 at high water, and 8 at low water. The mean for the low water was 1·02464, the extremes 1·02478 and 1·02435. The only case of special interest is one on the 12th. On that day there was a severe thunderstorm with exceedingly heavy rain for several hours. In the middle of it, during low water, when the quarry was land-locked, a bottle was filled holding its mouth just under the surface, and another at a depth of six inches. The result was that the extreme surface layer of water was found to have a density of 1·02394, the other of 1·02405, a difference of 0·00011. The high water figures differed only from 1·02405 to 1·02496, the mean being 1·02461.

During September 21 samples were taken at the quarry entrance at full tide; the mean density was 1·02446, the extremes being 1·02487 and 1·02296.

During this month a sample was taken up daily for some time at low water at a rock on the shore near the quarry. The mean density (12 observations) was 1·02461, the extremes 1·02506 and 1·02378, all higher than those at high water. The peculiarity of this result is emphasised when the curves are consulted, for there low tide is seen to produce a lower density beyond Inchkeith occasionally.

In October four samples only were collected; the mean density was 1·02319, the extremes being 1·02391 and 1·02193.

During November the density was determined on nine occasions; the mean was 1·02313, the highest 1·02390, and the lowest 1·02035.

In December samples were taken with greater regularity. They gave a mean of 1·02052, with extremes of 1·02449 and 1·00969 in 26 samples.

The figures for November and December are plotted in curve G (Plate V.), which shows the great irregularities that were observed.

It will be noticed by the dotted curve (Plate V., H) that since July the density has been continuously diminishing, though the larger curve shows that the diminution is very far from regular. On the 20th and 21st the surface water was almost fresh, indeed ice formed over the whole quarry at low water, although the temperature by a continuously immersed minimum thermometer did not fall below 32° Fahr. The bulb of this thermometer was 3 inches below the surface; and as there was a difference of over 3° Fahr. between the temperature at the depth of 6 inches, and when the bulb was just covered, at a time when the surface temperature was 34°, it is probable that the extreme surface layer was considerably colder than the minimum thermometer showed. The ice was about a centimetre thick, the upper third was hard, smooth, transparent, and quite free from air-bubbles, while the lower part was very spongy, seemingly composed of successive horizontal laminæ with water spaces between. Part of the ice collected was washed with distilled water, allowed to drain, then melted, and the chlorine determined by titration:—

	I.	II.	Mean.
Grammes per litre,	0·594	0·581	0·587

It is unfortunate that, owing to arrears of work consequent on illness, I did not determine the density of the water collected on the 20th and 21st until the 25th. The two bottles had in the interim been frozen solid during a cold night (22nd); they were found next morning with the cords securing the stopper burst, the stoppers lying on the table at a little distance, and the ice protruding above the neck of each. On the 25th the water in the quarry was as salt as usual, and of course it was too late to ascertain to what depth the freshening on the two days had extended.

It was suggested that the infiltration of fresh water from springs might lower the density of the surface water in the quarry, and so account for the peculiar temperature phenomena which have been observed there,* but on the 20th and 21st, and for several days before and after, all the sides of the quarry were sheathed in ice, the temperature of the air was below the freezing point, and, of course, at the time of sampling the tide was full. At low tide in August the water *in* the quarry was almost always found a little salter than at high water, showing the effect of evaporation to be greater than that due to springs.

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* *Proc. Roy. Soc. Ed.*, xii. 927.

On the 30th December the density was ascertained at several points at the time of high water, with the result that near a spring which falls into the quarry it was a little lower than at the entrance on the surface, and distinctly lower than at the bottom of the quarry, while half a mile outside in the firth the water was salter than near the shore (Table X.):—

TABLE X.

Position.	Depth.	Depth of Sample.	Density at 15°·56.	Temperature of Water.
Close to spring, . .	1 fathom.	Surface.	1·02372	39°·2 Fahr.
At "Ark," . .	7½ fathoms.	Bottom.	1·02393	39°·3 "
N.W. buoy in quarry,	8 fathoms.	Bottom.	1·02392	39°·4 "
Quarry entrance, .	1½ fathom.	Surface.	1·02382	39°·3 "
Half a mile outside } .	3½ fathoms.	Surface.	1·02436	40°·7 "
Due north, . } .	3½ fathoms.	Bottom.	1·02449	40°·1 "

The figures for all the quarry observations are given in Table XI.

TABLE XI.

Date.	No. of Sample.	Weather.	Water.	Tide.	Density at 15°·56.	Alkalinity.
July 29.	37	Dull—Fine.	Clear.	High Water.	1·02454	.
„ 30.	38	„	Muddy.	„	469	.
„ 31.	39	„	„	„	469	.
Aug. 1.	44	„	...	„	443	.
„ 1.	45	Sunny, Wind E.	...	Low Water.	455	.
„ 2.	46	Sunny, high W.	Very muddy	High Water.	463	.
„ 4.	48	Clear, W.	Clear.	Low Water.	440	.
„ 5.	49	Bright.	„	„	459	.
„ 5.	50	Sunny, S.W.	Muddy.	High Water.	442	.
„ 6.	51	Bright, S.	Clear.	Low Water.	435	.
„ 7.	52	Sunny, N.E.	...	„	471	.
„ 8.	53	Mist, N.E.	Muddy.	„	470	.
„ 8.	54	Clear, N.E.	...	High Water.	468	.
„ 9.	55	Cloudy, E.	...	Low Water.	469	.
„ 9.	56	„	Muddy.	High Water.	492	.
„ 11.	57	Sunny, Calm.	„	„	468	.
„ 12.	58	Thunder and Rain.	Very muddy	Low Water.	405	.
„ 13.	60	Sunny, S.E.	...	„	478	.
„ 13.	61	...	...	High Water.	464	.
„ 14.	62	Fine, S.	Muddy.	Low Water.	474	.
„ 15.	64	Clouds, Rain.	...	High Water.	480	.
„ 16.	65	Strong W.	...	„	466	.
„ 18.	66	Clear, Calm.	...	„	470	.
„ 21.	67	N.E. gale.	...	„	496	.
Sept. 1.	78	Calm.	Clear.	„	379	.
„ 1.	79	„	...	Low Water.	378	.

TABLE XI.—*continued.*

Date.	No. of Sample.	Weather.	Water.	Tide.	Density at 15°·56.	Alkalinity.
Sept. 2.	80	Clear, S.W.	...	High water.	418	.
,, 3.	81	,,	Sandy.	Low water.	472	.
,, 3.	82	Calm, Clear.	Clear.	High water.	440	.
,, 4.	83	Calm, Dull.	,,	Low water.	475	.
,, 4.	84	Calm, light Br.	,,	High water.	456	.
,, 5.	85	Bright, W.	Sandy.	Low water.	487	.
,, 5.	86	Bright, W. gale.	,,	High water.	472	.
,, 8.	87	Calm.	Very sandy.	Low water.	473	.
,, 8.	89	,,	Sandy.	High water.	454	.
,, 9.	90	,,	Clear.	Low water.	402	.
,, 9.	91	,,	...	High water.	458	.
,, 10.	98	,,	...	Low water.	472	50·4
,, 10.	99	Clear.	Clear	High water.	482	.
,, 11.	100	Calm, Cloud.	...	,,	476	.
,, 12.	101	Fog, N.E.	...	,,	487	.
,, 13.	102		...	,,	482	.
,, 15.	103	Clouds, N.E.	Clear.	,,	482	.
,, 15.	104	Clouds, N.E. gale.	Very muddy	Low water.	467	.
,, 16.	105	Thick Mist, N.E.	Muddy.	High water.	466	.
,, 17.	106	Clear, N.E.	Clear.	,,	458	.
,, 17.	107	Clear, Calm.	Sandy.	Low water.	467	.
,, 18.	108	...	...	High water.	465	.
,, 19.	109	Calm, Mist.	...	Low water.	440	.
,, 19.	110	Calm, Haze.	Clear.	High water.	466	.
,, 22.	138	Strong S.	Sandy.	Low water.	506	.
,, 22.	139	Strong S.W.	Clear.	High water.	476	.
,, 23.	140	,,	Sandy.	Low water.	497	.
,, 23.	141	Rain, S.W.	Clear.	High water.	469	.
,, 26.	142	Calm.	,,	,,	460	.
,, 29.	152	Calm, S.W,	...	,,	338	.
,, 30.	154	Squally, S.W,	...	,,	360	.
,, 30.	155	,,	...	Low water.	389	.
Oct. 1.	156	Strong S.W.	Sandy.	High water.	388	.
,, 2.	157	Fine, Calm.	...	,,	391	.
,, 3.	158	Bright, S.W.	...	,,	303	.
,, 4.	159	...	...	,,	193	.
Nov. 21.	217	Clear, Calm.	Muddy.	,,	366	53·5
,, 26.	218	Dull, W.	Clear.	,,	390	46·0
,, 27.	219	,,	Muddy.	,,	381	51·8
,, 28.	220	Clear, W.	Clear.	,,	347	46·9
,, 29.	221	Bright, W.	,,	,,	345	45·2
,, 30.	222	Dull, E.	,,	,,	376	45·7
Dec. 1.	223	Dull, S.	,,	,,	418	52·7
,, 2.	224	Bright, W.	,,	,,	449	46·9
,, 3.	225	Bright, S.W.	,,	,,	1·01850	33·9
,, 4.	226	Clear, E.	Muddy.	,,	1·01925	44·0
,, 5.	227	Dull, W.	Clear.	,,	1·02444	52·7
,, 6.	228	,,	Muddy.	,,	1·01860	34·1
,, 7.	229	Dull, N.E.	Clear.	,,	1·01875	37·3
,, 8.	230	Dull, S.W.	Muddy.	,,	1·01858	39·7
,, 9.	231	Dull, W.	Clear.	,,	1·02160	41·1
,, 10.	232	S.W.	,,	,,	1·01780	38·2
,, 11.	233	Clear, S.W.	Muddy.	,,	1·02002	.
,, 12.	234	,,	,,	,,	1·01911	42·3
,, 13.	235	Bright, S.W.	Clear.	,,	1·02202	43·9
,, 14.	236	Clear, S.W.	,,	,,	243	46·5
,, 15.	237	Bright, S.W.	,,	,,	219	.
,, 16.	238	Clear, S.W.	,,	,,	247	.
,, 17.	239	Clear, W.	,,	,,	226	.
,, 18.	240	Dull, S.W.	,,	,,	289	.
,, 19.	241	Clear.	Muddy.	,,	1·01906	.
,, 20.*	242	Clear, N.	Clear.	,,	1·01259	31·5
,, 21.*	243	Dull, W.	,,	,,	1·00969	25·6
,, 22.	244	,,	,,	,,	1·02161	38·2
,, 23.	245	Dull, N.	,,	,,	159	.
,, 24.	246	,,	,,	,,	283	.
,, 27.	262	Dull, S.W.	,,	,,	275	48·4
,, 30.	266	Dull, Calm.	Muddy.	,,	382	.

* These samples froze in the bottle before the density was determined

A probable explanation of the freshness of the quarry surface water at times is that the River Almond when in flood, or whether wind is in certain directions, dilutes the water along the shore to the east of it. This would also account for the water near the quarry being denser at low tide, for then the Almond runs into the firth on the west of Cramond Island, and so carries its freshening influence in another direction (see Plate III.).

A number of glass floats, coloured distinctively, were thrown into the Almond on several occasions just at the turn of the tide, immediately after high water, but none of them were seen again, though all the boys of the neighbourhood were on the alert along the shore, a small reward having been offered for each float found.

5. *The Bottom Water of the Firth.*

As there was considerable time lost in devising and testing a suitable means of collecting samples of bottom water, the number of cases for consideration is small.

It may be generally stated, that the part of the firth east of Inchkeith is the region where the difference between the density of surface and bottom water is least, and that the difference decreases steadily towards the May. Towards Alloa, on the other hand, the differences in the density between surface and bottom water are great, but they are greatly influenced by the tide.

Table XII. gives details of 19 comparisons between bottom and surface water.

At the Hen and Chickens Buoy, off Grangemouth, the depth is only 5½ fathoms, but the salinity is very much greater at the bottom than at the surface. The difference is least observed at low water, but as the flood tide sets in it appears to increase, and then to fall off again as the ebb commences. The only divergence from this rule noticed was on November 11, when the rivers were all much flooded, and the current very rapid. It would appear that these observations confirm the theory that sea water ascends rivers along the bottom under the opposite current of fresh water.

At Inchgarvie, where there is a deep depression (over 40 fathoms) in the middle of the stream, the bottom water is always much salter than that at the surface, and the result is most noticeable at low water. The difference is usually like that between

TABLE XII.—*Bottom Water.*

Date.	Samples.	Tide.	Depth.	Density.			Alkalinity.		
				Bottom.	Surface.	Dif.	Bot.	Sur.	Dif.
					At Hen and Chickens (III.)				
19-6-84	20 and 21	low water	2 fm.	1·01876	1·01850	26	...	...	...
8-10-84	183 and 184	2 hr. fld.	4 „	1·02041	1·01693	348	...	...	...
11-11-84	206 and 208	1 hr. ebb	5½ „	1·02158	1·01755	403*	44·7	34·3	10·4
11-11-84	207 and 209	5 hr. ebb	5½ „	1·02040	1·01084	956*	47·4	28·5	16·9
					At Inchgarvie (V.)				
19-6-84	22 and 24	1 hr. fld.	25 fms.	1·02478	1·02424	54	...	...	...
31-7-84	41 and 43	5 hr. ebb	31 „	1·02455	1·02324	131	...	...	...
18-9-84	129 and 133	½ ebb	35 „	1·02448	1·02425	23	56·9	47·9	9·0
8-10-84	178 and 179	low water	40 „	1·02437	1·02241	196	...	...	...
11-11-84	200 and 202	high water	32 „	1·02508	1·02029	479*	51·8	46·5	5·3
11-11-84	201 and 203	low water	35 „	1·02476	1·01863	613*	45·0	42·1	2·9
27-12-84	259 and 260	high water	38½ „	1·02509	1·02351	158	53·9	48·6	5·3
					At Inchkeith (VII.)				
18-9-84	119 and 123	¾ ebb	15½ fm.	1·02491	1·02481	10	50·4	48·2	2·2
7-10-84	160 and 161	low water	13½ „	1·02517	1·02519	2	...	...	...
8-10-84	174 and 175	¾ ebb	10 „	1·02501	1·02494	7	...	...	...
25-12-84	247 and 248	2 hr. ebb	17 „	1·02531	1·02470	61	48·6	48·6	0
					At Station IX.				
11-6-84	13 and 15	5 hr. ebb	22 fm.	1·02424	1·02417	7	...	...	...
					At Station XI.				
4-6-84	1 and 3	½ ebb	23 fm.	1·02542	1·02528	14	...	...	...
					At May Island (XII.)				
11-6-84	16 and 17	2 hr. fld.	7 fm.	1·02532	1·02443	89	...	...	...
7-10-84	167 and 168	¾ fld.	9 „	1·02562	1·02547	15	...	...	...

* River in flood.

1·0240 at the bottom and 1·0220 at the surface. On November 11 the bottom density was 1·02476 at low water, the surface was only 1·01863, a difference of 0·00613. This was evidently due to the unusual rapidity of the surface current caused by the swollen rivers, being sufficient to retard the process of diffusion between the two kinds of water. As an ordinary thing, the bottom water at Inchgarvie is nearly as salt as that at Inchkeith, while the surface water is much fresher; in fact, in a heavy flood there is river water on the surface and sea water below it. The following comparison of water from half the depth with that at the bottom and surface shows that the denser water extends at any rate to half the depth, so that one would be inclined to view the heavy sea water filling up the hollow as part of the river bed, over which the fresher upper water flows. Experiments with a current gauge at various depths would be certain to give much valuable information on this subject.

	a.	*b.*	*c.*
Bottom,	1·02478	1·02455	1·02448
Half-Depth,	1·02461	1·02440	1·02426*
Surface,	1·02424	1·02324	1·02425

The salinity of the bottom water was never found less than that of the surface.

6. *The Alkalinity of the Firth of Forth.*

Table XIV. gives particulars of 98 determinations of alkalinity arranged according to magnitude.

Speaking roughly, it shows that waters having an alkalinity under 40 (that is in which there is less than 0·04 grammes of carbonic acid as carbonate of lime per litre) have a density under 1·0200, alkalinities under 25 correspond to densities under 1·0100. The only strikingly anomalous case is that of sample No. 189, a bottom water from off Alloa, when the river was low and very dirty. The density was 1·00146, the alkalinity 47·9, which usually corresponds to a density of 1·024. The presence of sewage in the river might account for this observation to some extent, but more probably it is due to the presence of particles of calcium carbonate, to which cause the very exceptional alkalinities of Nos. 94 and 70 may also be referred.

* Uncertain.

With an alkalinity between 40 and 50 water has a density between 1·024 and 1·025 as a general rule, and when the alkalinity is over 50 the density is almost invariably over 1·025.

There are a number of exceptions apparent on looking over these tables, and the exceptions are too numerous to be accidental.

To discuss the bearings of alkalinity on the other properties of the water with any degree of satisfaction, it would be necessary to translate the number representing alkalinity per litre into that representing alkalinity per 100 parts of total salts. If it could be correctly assumed that the composition of the total salts is constant, like the composition of ocean-water salts, this could easily be done by calculation from the density. But it is evident from purely *à priori* considerations, that such an assumption cannot be made, for the composition of the dissolved salts of river water is quite different from that of the salts of sea water, and consequently estuary water is not a solution of sea-water salts diluted more and more with pure water, but a solution of sea-water salts mixed more and more largely with a much more dilute solution of salts in a different proportion.

In consequence of inability to compare the alkalinites with the total salts, it is impossible to form a correct idea of the difference between bottom and surface alkalinities, as this difference may be entirely due to the different salinity of the water.

The following table (XIII.) shows that the difference in alkalinity, like that in salinity between surface and bottom water, is greatest at the highest point in the river, and decreases rapidly as the sea is approached.

TABLE XIII.

	Bottom.	Surface.	Difference.	Mean difference.
Station III., near Grangemouth.				
	44·7	34·3	10·4	...
	47·4	28·5	18·9	14·7
Station V., near Inchgarvie.				
	56·9	47·9	9·0	
	51·8	46·5	5·3	
	45·0	42·1	2·9	
	53·9	48·6	5·3	5·6
Station VII., near Inchkeith.				
	50·4	48·2	2·2	
	48·6	48·6	0·0	1·1

For particulars as to density, &c., see Table XII.

TABLE XIV.

Water Sample.	Alkalinity.	Density at 15·°56.	Position.	State of Tide.
212	1·2	0·99923	I	½ Ebb (*f*).
213*	4·8	0·99939	,,	,, (*f*).
210	11·1	1·00474	II.	¼ Ebb (*f*).
211	12·1	070	,,	¾ Ebb (*f*).
243	25·6	969	Q	High water.
207	28·5	1·01084	III.	Low water.
242	31·5	259	Q	High water.
225	33·9	850	,,	,,
228‡	34·1	860	,,	,,
206	34·3	755	III.	,, (*f*).
205	37·1	758	IV.	Low water (*f*).
229	37·3	875	Q	High water.
204	38·2	650	IV.	,, (*f*).
232	38·2	780	Q	,,
244	38·2	1·02160	,,	,,
230‡	39·7	1·01858	,,	,,
231	41·1	1·02160	,,	,,
201	42·1	1·01863	V.	Low water (*f*).
234‡	42·3	911	Q	High water.
199	43·9	953	VI.	,, (*f*).
235	43·9	1·02202	Q	,,
226‡	44·0	1·01925	,,	,,
208*	44·7	1·02158	III.	,, (*f*).
203*	45·0	476	V.	Low water (*f*).
221	45·2	345	Q	High water.
222	45·7	376	,,	,,
218	46·0	390	,,	,,
198	46·5	091	VI.	,, (*f*).
200	46·5	029	V.	,, (*f*).
236	46·5	243	Q	,,
220	46·9	347	,,	,,
224	46·9	449	,,	,,
209*	47·4	040	III.	Low water (*f*).
215‡	47·4	346	Q	High water.
190	47·5	427	VII.	¼ Ebb (*f*).
133	47·9	425	V.	½ Ebb.
189*	47·9	1·00146	I.	¼ Flood (*f*).
214	47·9	1·02230	Q	High water.
261	47·9	297	VI.	¼ Ebb.
249	48·1	521	VIII.	½ Ebb.
123	48·2	481	VII.	¾ Ebb.
191	48·2	450	VIII.	½ Ebb (*f*).
262	48·4	275	Q	High water.
247	48·6	470	VII.	¼ Ebb.
248*	48·6	531	,,	,,
259	48·6	351	V.	High water.
250	48·9	551	IX.	½ Ebb.

Q stands for "Quarry Mouth," and (*f*) for "river in flood."

*** Bottom sample.**
† Sample from intermediate depth.
‡ Water noted as full of particles.

TABLE XIV.—*continued.*

Water Sample.	Alkalinity.	Density at 15·°56.	Position.	State of Tide.
196	49·0	467	VIII.	¼ Flood (*f*).
115*	49·2	498	VII.	¾ Flood.
194	49·7	563	X.	Low water (*f*).
195	49·7	495	IX.	,, (*f*).
216‡	50·1	035	Q	High water.
197	50·1	380	VII.	¼ Flood (*f*).
112	50·2	492	VI.	¼ Ebb.
98	50·4	472	Q	Low water.
119*	50·4	491	VII.	¾ Ebb.
124	50·4	472	VI.	High water.
125	50·4	469	,,	¾ Ebb.
128	50·4	461	V.	High water.
113	50·8	479	VI.	¾ Ebb.
95	51·1	526	VII.	High water.
111	51·1	508	,,	Low water.
116*	51·1	505	,,	¾ Flood.
117†	51·1	510	,,	,,
118	51·1	505	,,	,,
254	51·1	545	X.	Low water.
72	51·3	552	VII.	,,
131†	51·3	426	V.	½ Ebb.
192	51·3	508	IX.	,, (*f*).
193	51·3	557	X.	¾ Ebb (*f*).
258	51·3	410	VI.	High water.
69	51·5	500	VII.	¼ Ebb.
114	51·5	447	V.	¾ Ebb.
120*	51·5	493	VII.	¾ Ebb.
97*	51·7	528	,,	High water.
202*	51·8	508	V.	Low water.
219‡	51·8	381	Q	High water.
96†	51·9	531	VII.	,,
73†	52·0	554	,,	Low water.
122	52·2	493	,,	¾ Ebb.
251	52·3	545	X.	,,
223	52·7	418	Q	High water.
227	52·7	444	,,	,,
253*	53·0	556	XI.	¾ Ebb.
71†	53·3	533	VII.	¼ Ebb.
74*	53·3	554	,,	Low water.
68	53·5	498	,,	¼ Ebb.
217	53·5	366	Q	High water.
257	53·7	458	VII.	¼ Flood.
93†	53·8	542	St Abb's Head.	...
260*	53·9	509	V.	High water.
255	54·0	465	IX.	Low water.
252	54·9	552	XI.	¾ Ebb.
92	56·0	547	St Abb's Head.	...
256	56·4	502	VIII.	Low water.
129	56·9	448	V.	½ Ebb.
94*	60·0	551	St Abb's Head.	...
70*	63·6	519	VII.	¼ Ebb.

Monday, 19*th January* 1885.

A. FORBES IRVINE, Esq., Vice-President,
in the Chair.

The following Communications were read:—

1. Notes on the Chemical Composition of the Cobalt and Nickel Ores of New Caledonia, with some Remarks on the Properties and Uses of Metallic Nickel and Oxides of Cobalt. By J. B. Readman, Esq.

The island of New Caledonia, from which these ores of nickel and cobalt are derived, is situated in the South Pacific Ocean, distant about 600 miles east from Queensland, Australia. The island is about 200 miles long by about 30 miles broad, and is extremely mountainous. New Caledonia belongs to the French, and it is there that they have established one of their principal penal settlements.

The nickel ores of New Caledonia were discovered rather more than twenty years ago by Garnier, in an exploring expedition of the island undertaken under the auspices of the French Government. The ores of cobalt were a more recent discovery.

Beginning with the deposits of cobalt ores in New Caledonia, I shall mention briefly—(1) The kind of deposits of cobalt ore; (2) their physical state; and (lastly) their chemical composition, with some of the properties and the uses of the oxides of cobalt.

1. *The kind of Deposits of Cobalt Ore.*—The surface deposits of cobalt ore are found in the midst of certain aluminous ferruginous masses, which seem to come from the decomposition of eruptive aluminous ferruginous rocks plentifully scattered in these districts. These masses are very much developed at the surface, and form immense ferruginous plateaux, which sometimes crown the top of the principal serpentine upheavals, but which occupy specially the ridges of the less elevated ranges of hills. This peculiarity causes the cobalt ores of New Caledonia to be found generally at low altitudes and not far from the sea, where the branches of the great

mountain chain of the interior die away. The cobalt ores of New Caledonia habitually contain veins of chrome ore, which appear to come from chrome ore contained in the subjacent serpentine.

2. *The Physical Condition of the Cobalt Ore.*—The ore from the ferruginous masses containing cobalt is solid, round, and sometimes a little hard. I have here some specimens that show the appearance it presents on the ground. It resembles iron ore, rotten or scoriaceous, the cavities of which are filled with bluish-black cobaltiferous manganese.

3. *The Chemical Composition of the Cobalt Ores, &c.*—The average percentage of the ore, taken from a large number of analyses, runs about 3 to 5 per cent. oxide of cobalt, along with about 1¼ per cent. of oxide of nickel. Besides these two oxides, the ore contains binoxide and protoxide of manganese, peroxide of iron, alumina, lime, magnesia, silica, chrome ore, and water. The following are the detailed analyses:—

	Mean.						
Oxide of cobalt,	(5·11)	3·48	3·41	4·16	5·56	6·40	7·76
Oxide of nickel,	(1·46)	1·05	1·32	1·64	1·48	1·64	1·64
Binoxide of manganese,		23·34	18·78	22·37	15·94	28·20	34·10
Protoxide of manganese,		2·12	2·33	0·82	3·40	4·90	3·50
Sesquioxide of iron, .		15·71	14·39	10·06	16·06	13·04	8·91
Alumina, . . .		21·20	31·73	13·44	10·30	16·76	14·29
Lime, . . .		2·68	1·95	0·32	1·53	1·70	1·59
Sulphuric acid, . .		...	0·42	...	...	...	...
Magnesia, . . .		2·16	Trace	...	0·70	0·70	0·79
Silica and chrome ore, .		9·20	1·81	30·00	23·00	5·32	2·20
Combined water, .		17·84	19·22	17·19	9·23	12·14	9·42
Moisture, . . .		1·70	4·63		12·86	9·20	15·80
		100·48	99·99	100·00	100·06	99·80	100·00

It will be seen from the analysis of the six samples of cobalt ore (and which are representative analysis of several hundred tons), that the composition of the ore varies very considerably,—the silica and chrome ore being as high as 30 per cent. in one representative sample, and as low as 1·81 per cent. in another.

In the numerous samples submitted to analysis, I have not heard of copper, sulphur (either in free state or as a sulphide), or arsenic, being found in these ores of cobalt.

It has not been found advisable to attempt to utilise as yet any portion of the constituent of these cobalt ores except the cobalt and the nickel they contain, so it is needless at present to mention them further.

Cobalt is almost entirely consumed in the arts in the state of oxide, and there are two fairly well-defined oxides of this metal that are so employed. The first of these, or what is known as the ordinary *black oxide of cobalt* of commerce, is a sesquioxide (Co_2O_3), corresponding to the ferric oxide Fe_2O_3; and the other oxide is partly protoxide and partly sesquioxide of cobalt, corresponding to, though not so well defined, as the magnetic oxide of iron Fe_3O_4. This oxide is known in the arts as *prepared oxide of cobalt*, and is made from the black or sesquioxide by heating to whiteness for some time, and thus depriving it of a part of its oxygen.

In physical character the two oxides of cobalt are somewhat unlike, the higher oxide being *blacker* than the lower oxide, and less *dense*, and though the former is apparently much lighter, I was surprised to find that the specific gravity of the two oxides are not very widely different, as will be seen from the following results:— The specific gravity of two different representative samples of black oxide being 5·82 and 5·82 respectively, while the specific gravity of four different samples of prepared oxide were found to be 5·93, 6·48, 6·38, and 6·12.

I have here two specimens of the cobalt oxides just described, and which have been produced from the cobalt ores of New Caledonia. The following are the analyses:—

Black Oxide of Cobalt.

Sesquioxide of cobalt,	96·15 = 68·3 per cent. Co	96·91
Protoxide of nickel,	2·06	1·12
Sesquioxide of iron,	0·68	0·40
Tetroxide of manganese,	0·44	Nil.
Lime,	0·76	1·80
Magnesia,	Trace	0·50
Insoluble, silica, &c.,	Trace	0·16
	100·09	100·89

Analysis of Prepared Oxide of Cobalt.

	Per cent.	
Protoxide of cobalt (CoO),	*93·85	94·27
Protoxide of nickel,	1·79	1·17

* Equal to Sesquioxide of cobalt, Co_2O_2, 103·86
Do. Tetroxide of cobalt, Co_3O_4, 100·52
Do. Metallic cobalt, Co, 73·84

Sesquioxide of iron,	1·04	1·08
Sesquioxide of manganese,	0·16	0·14
Lime,	0·32	0·60
Magnesia,	0·25	0·40
Silica,	0·32	0·44
Oxygen by difference,	2·27	1·90
	100·00	100·00

It will be seen from the analysis of the prepared oxide of cobalt that it is not a well-defined oxide, but is rather a combination of protoxide with sesquioxide.

The principal use of black oxide of cobalt is in the manufacture of pottery ware, where it is employed to counteract the yellow colour the ware would otherwise possess after the process of firing,—the cobalt here playing the part of the blue of the laundry. So intense is the colouring power of cobalt that about 6 oz. to the ton of china clay is sufficient, and if more than about 1 lb. to the ton of clay be used, the ware has a distinct blue colour. I have received from a large pottery two specimens of china ware, one of which has had the usual admixture of cobalt, while another one has had no cobalt added to the clay from which the dish was made. In daylight a very decided yellow tint is visible in the latter.

The other uses of *black* oxide of cobalt are in colouring glass, and in the manufacture of cobalt ultramarine.

The principal use of *prepared* oxide of cobalt is as a pigment, especially in painting china (I have here a familiar specimen of a plate painted with this pigment), cobalt being the only blue colour that will stand the high temperature required in this case.

Up to the present time I have not heard of cobalt being found in any well-defined veins in New Caledonia—in this matter cobalt differs from the ores of nickel, which are found also in the island.

The nickel ores of New Caledonia contain no cobalt, but the cobalt ores, as we have seen from the analyses, contain a considerable percentage of nickel compared with the cobalt present.

It is rather troublesome to separate all the nickel from the oxide of cobalt in the process of refining, and in no sample that I have examined is the nickel entirely absent in the finished oxide of cobalt.

Nickel exists in the ores of New Caledonia as a silicate,—possibly a double silicate of nickel and magnesia,—and this ore is probably one of the most easily refined of all the ores of nickel. This is readily seen from the following analyses of six representative samples :—

Oxide of nickel,	7·00	11·08	12·68	18·04	19·00	24·28
Oxide of iron, .	4·47	7·34	7·14	0·88	6·60	3·80
Alumina, . .	2·53	1·56	1·50	1·24		
Lime, . .	2·30	1·90	0·72	Trace	Trace	0·58
Magnesia, . .	31·42	22·50	21·80	16·37	16·30	15·75
Silica, . .	40·96	37·96	44·00	45·74	44·00	40·24
Water, . .	12·00	17·30	12·16	17·28	20·00	16·21
	100·68	99·64	100·00	99·55	99·90	100·86

The average ores worked contain about 10 per cent. of a metallic nickel. I have here as representative a set of specimens of the nickel ores as could be obtained. It has a green colour, and was mistaken for an ore of copper at one time.

The refined nickel is usually sent into the market either in shape of small cubes or round cakes. I have here specimens of each of these. The nickel rounds are prepared by mixing, intimately, the oxide of nickel refined from the ore, along with wood charcoal, and after stamping into the round shape (shown in the specimen), the cakes are dried, and the oxide is reduced to the metallic state at a high temperature, the round cakes still retaining their shape. Nickel made in this way is chiefly employed in the manufacture of German silver, or other alloys of nickel and copper; it is brittle, and has a distinct crystalline appearance on being broken. Before proceeding to speak of malleable nickel and other uses of the metal and its alloys, I shall give a few of the analyses that have been made of the oxide and of the metallic nickel manufactured from the ores of New Caledonia. I may here mention that these analyses are representatives of a great many hundred tons of refined oxide and metallic nickel derived from New Caledonia, which is believed to be the largest source of nickel supply in the world.

Analyses of Oxide of Nickel.

Nickel, . . .	77·51	77·67
Copper, . . .	None	None
Iron, . . .	0·54	0·47
Oxygen, . . .	21·30	21·50
Impurities, . .	0·65	0·34
	100·00	99·98

Four Analyses of Nickel Cakes.—Metallic nickel 98·25 per cent., 98·82 per cent., 98·83 per cent., 98·37 per cent. I am not able at present to give greater detail in the last analysis, the difference, however, is made up of some carbon, iron, and silicon.

Of recent years a process has been patented to make nickel malleable, and render the metal suitable for rolling and stamping. This process consists in adding to the fused nickel contained in a crucible a small quantity of metallic magnesium, the effect of this, according to some authorities, is to deprive the nickel of a minute quantity of oxygen. The metal that this sheet was made from, and the various nickel articles here, have been manufactured from nickel treated in this way. The importance of malleable nickel for many purposes in the arts is very great.

Pure nickel is not a readily oxidisable metal, and its resistance to the action of the atmosphere, and also to the action of both fresh and salt water, admits of its being ranked in these respects with the precious metals.

This valuable property of not being readily tarnished or oxidised offers special inducements to employ nickel for the exposed parts of machinery and engines, for surgical, optical, and scientific instruments and apparatus, for artistic and ornamental work of all sorts, in jewellery, and in a great variety of other ways.

Another important property of nickel is its whitening effect upon copper when alloyed with it, forming the well known "German" or "nickel silver." Some very beautiful alloys of nickel, copper, and tin have been quite recently obtained, and these alloys have received the name of "New Caledonia silver." This alloy is peculiarly white in colour, whiter indeed than pure nickel itself. It is also very malleable. I have obtained specimens of sheets made of this composition, and likewise of some articles, which may be of interest to this Society, made from the same alloy.

Another very important use of nickel is in the sheathing of ships; perhaps sufficient trials have not yet been made to speak confidently, but I may perhaps be allowed to quote the remarks of a gentleman present at the docking of a ship on which some experiments had been made. He was present at the docking of the "Roma," and writes as follows:—

"The whole ship, as far as it had been in the water, was covered with a band of sea-weed and shells for a thickness of several inches; the nickel plates were, however, completely free of all growth, and quite clean, the only alteration in appearance being a slight violet discoloration, such as tarnished silverplate. Otherwise they were without any dirt or slime, so that they could have been written on with a lead pencil. The sheets seemed to be bright eyes or windows, and the nickel plates were not blistered or damaged in any way. The plates having been brushed were as clean as new ones. It should be remembered that the 'Roma' was at sea for eleven months, and that she was in some of the worst places—such as the Straits of Magellan—for the accretion of sea-weed and barnacles."

Recently culinary and other vessels made of sheet iron, coated inside and out with thin sheets of malleable nickel, have been the object of manufacture. These articles, of which I have here some specimens, are indistinguishable of course from vessels made of pure nickel throughout. (I have here some examples of the latter for comparison.) The object of coating these vessels is to save expense compared with the pure metal. Lastly, I may just mention that another important outlet for nickel is in the coinage. Many countries have now a nickel coinage, and it is probable the use of nickel in this direction will be soon largely extended.

In conclusion, I should like to give a few details of some experiments I have recently made in regard to the solubility of nickel in acids, and its behaviour in some other ways.

First, in regard to the solubility of nickel in acids. I began with acetic acid, and the treatment in this acid was repeated very much in the same way as in case of other acids—that is, the nickel was boiled in a large quantity of acid of various strengths, and allowed to remain under this treatment four to five days.

1. In acetic acid, 0·21 per cent. of nickel dissolved.

2. In tartaric acid, the nickel was not acted upon.

3. In a mixture of solution of citric and tartaric acids, the nickel upon the treatment as described lost 0·08 per cent.

4. In oxalic acid, the loss amounted to 0·14 per cent.

5. In cold dilute hydrochloric acid, no loss was sustained (the HCl sol. was 1 to 6); but on heating the solution the nickel dissolved.

6. In cold dilute sulphuric acid (1 to 4), the nickel lost 0·14 per cent., and behaved on heating as with the last acid.

When nickel articles, such as those before you, are heated over an Argand or Bunsen burner, a thin unweighable coating of oxide is formed (which can be removed by sand-paper), the nickel vessel becoming very dark in colour.

Caustic soda can be fused in a nickel basin with scarcely any change in weight. In one experiment, where the caustic soda was fused for one hour in a small nickel basin, the gain in weight amounted to 0·013 per cent.

These, then, are a few of the experiments that I have been able to make with this metal; some of them proving the value of nickel for many purposes to which it has hitherto not been applied.

2. Note on a Singular Passage in the *Principia*, By Professor Tait.

In the remarkable *Scholium*, appended to his chapter on the Laws of Motion, where Newton is showing what Wren, Wallis, and Huygens had done in connection with the impact of bodies, he uses the following very peculiar language:—

"Sed et veritas comprobata est a *D. Wrenno* coram *Regiâ Societate* per experimentum Pendulorum, quod etiam *Clarissimus Mariottus* Libro integro exponere mox dignatus est."

The last clause of this sentence, which I had occasion to consult a few days ago, appeared to me to be so sarcastic, and so unlike in tone to all the context, that I was anxious to discover its full intention.

Not one of the Commentators, to whose works I had access, makes any remark on the passage. The Translators differ widely.

Thus Motte softens the clause down into the trivial remark "which Mr Mariotte soon after thought fit to explain in a treatise entirely on that subject."

The Marquise du Chastellet (1756) renders it thus:—

"...... mais ce fait *Wrenn* qui les confirma par des Expériences faites avec des Pendules devant la Sociétè Royale: lesqelles le célèbre *Mariotte* a rapportées depuis dans un Traité qu'il a composé exprès sur cette matière."

Thorp's translation (1777) runs:—

"which the very eminent Mr Mariotte soon after thought fit to explain in a treatise entirely upon that subject."

Finally, Wolfers (1872) renders it thus:—

"der zweite zeigte der Societät die Richtigkeit seiner Erfindung an einem Pendelversuche, den der berühmte Mariotte in seinem eigenen Werke aus einander zu setzen, für würdig erachtete."

Not one of these seems to have remarked anything singular in the language employed. But when we consult the "entire book" in which Mariotte is said by Newton to have "expounded" the result of Wren, and which is entitled *Traité de la Percussion ou Choc des Corps*, we find that the name of Wren is not once mentioned in its pages! From the beginning to the end there is nothing calculated even to hint to the reader that the treatise is not wholly original.

This gives a clue to the reason for Newton's sarcastic language; whose intensity is heightened by the contrast between the *Clarissimus* which is carefully prefixed to the name of Mariotte, and the simple *D.* prefixed, not only to the names of Englishmen like Wren and Wallis, but even to that of a specially distinguished foreigner like Huygens.

Newton must, of course, like all the scientific men of the time (Mariotte included), have been fully cognizant of Boyle's celebrated controversy with Linus, which led to the publication, in 1662, of the *Defence of the Doctrine touching the Spring and Weight of the Air.* In that tract, Part II. Chap. 5, the result called in Britain *Boyle's Law* is established (by a very remarkable series of experiments) for pressures less than, as well as for pressures greater than, an atmosphere; and it is established by means of the very form of apparatus still employed for the purpose in lecture demonstrations. Boyle, at least, claimed originality, for he says in connection with the difficulties met with in the breaking of his glass tube:—

". . . . an accurate Experiment of this nature would be of great importance to the Doctrine of the Spring of the Air, and has not been made (that I know) by any man. . . ."

In Mariotte's *Discours de la Nature de l' Air*, published FOURTEEN years later than this work of Boyle, we find no mention whatever of

Boyle, though the identical form of apparatus used by Boyle is described. The whole work proceeds, as does that on *Percussion*, with a calm ignoration of the labours of the majority of contemporary philosophers.

This also must, of course, have been perfectly well known to Newton:—and we can now see full reason for the markedly peculiar language which he permits himself to employ with reference to Mariotte.

What was thought of this matter by a very distinguished foreign contemporary, appears from the treatise of James Bernoulli, *De Gravitate Ætheris*, Amsterdam, 1683, p. 92.

"Veritas utriusque hujus regulæ manifesta fit duobus curiosis experimentis ab Illustr. Dn Boylio hanc in rem factis, quæ videsis in *Tractatu ejus contrà Linum, Cap.* V., cui duas Auctor subjunxit Tabulas pro diversis Condensationis et Rarefactionis gradibus."

In order to satisfy myself that Newton's language, taken in its obvious meaning, really has the intention which I could not avoid attaching to it, I requested my colleague Prof. Butcher to state the impression which it produced on him. I copied for him the passage above quoted, putting A. for the word *Wrenno*, and B. for *Mariottus;* and I expressly avoided stating who was the writer. Here is his reply:—

"I imagine the point of the passage to be something of this kind (speaking without farther context or acquaintance with the Latinity of the learned author):—

A established the truth by means of a (simple) experiment, before the Royal Society; later, B thought it worth his while to write a whole book to prove the same point.

I should take the tone to be highly sarcastic at B's expense. It *seems* to suggest that B was not only clumsy but dishonest. The latter inference is not certain, but at any rate we have a *hint* that B took no notice of A's discovery, and spent a deal of useless labour."

This conclusion, it will be seen, agrees exactly with the complete ignoration of Wren by Mariotte.

When I afterwards referred Prof. Butcher to the whole context, in my copy of the first edition of the *Principia*, and asked him whether the use of *Clarissimus* was sarcastic or not, he wrote—

"I certainly think so. Indeed, even apart from the context, I thought the *Clarissimus* was ironical, but there can be no doubt of it when it corresponds to *D.* Wren."

In explanation of this I must mention that, when I first sent the passage to Prof. Butcher, I had copied it from Horsley's sumptuous edition; in which the *D*s are omitted, while the *Clarissimus* is retained.

Alike in France and in Germany, to this day, the Law in question goes by the name of Mariotte. The following extracts, from two of the most recent high-class text books, have now a peculiar interest. I have put a word or two of each in Italics. These should be compared with the dates given.

"Diese Frage ist schon frühzeitig untersucht und zwar *fast gleichzeitig* von dem französischen Physiker Mariotte (1679) und dem englischen Physiker Boyle (1662)." Wüllner, *Lehrbuch der Experimentalphysik*, 1882, § 98.

"La loi qui régit la compressibilité des gaz à température constante a été trouvée *presque simultanément* par Boyle (1662) en Angleterre et par Mariotte (1676) en France; toutefois, si Boyle a publié le premier ses expériences, il ne sut pas en tirer l'énoncé clair que donna le physicien français. C'est donc avec quelque raison que le nom de loi de Mariotte a passé dans l'usage." Violle, *Cours de Physique*, 1884, § 283.

On this I need make no remark further than quoting one sentence from Boyle, where he compares the actual pressure, employed in producing a certain compression in air, with "what the pressure should be according to the *Hypothesis*, that supposes the pressures and expansions to be in reciprocal proportion." M. Violle has probably been misled by the archaic use of "expansion" for volume.

It must be said, in justice to Mariotte, that he does not appear to have *claimed* the discovery of any new facts in connection either with collision or with the effect of pressure on air. He rather appears to write with the conscious infallibility of a man for whom nature has no secrets. And he transcribes, or adapts, into his writings (without any attempt at acknowledgment) whatever suits him in those of other people. He seems to have been a splendidly successful and very early example of the highest class of what we now call the *Paper-Scientists.* Witness the following extracts from

Boyle, with a parallel citation from Mariotte of *fourteen* years' later date *at least.* The comparison of the sponges had struck me so much, in Mariotte's work, that I was induced to search for it in Boyle, where I felt convinced that I should find it.

"This Notion may perhaps be somewhat further explain'd, by conceiveing the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which, may indeed, like a little Spring, be easily bent or rouled up; but will also, like a Spring, be still endeavouring to stretch itself out again. For though both these Haires, and the Æreal Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by virtue of its structure) is endow'd with a Power or Principle of Selfe-Dilatation; by virtue whereof, though the hairs may by a Mans hand be bent and crouded closer together, and into a narrower room then suits best with the Nature of the Body, yet, whils't the compression lasts, there is in the fleece they composeth an endeavour outwards, whereby it continually thrusts against the hand that opposeth its Expansion. And upon the removall of the external pressure, by opening the hand more or less, the compressed Wooll doth, as it were, spontaneously expand or display it self towards the recovery of its former more loose and free condition till the Fleece hath either regain'd its former Dimensions, or at least, approached them as neare as the compressing hand, (perchance not quite open'd) will permit. The power of Selfe-Dilatation is somewhat more conspicuous in a dry Spunge compress'd, then in a Fleece of Wooll. But yet we rather chose to employ the latter, on this occasion, because it is not like a Spunge, an intire Body; but a number of slender and flexible Bodies, loosely complicated, as the Air itself seems to be."

And, a few pages later, he adds:—

". . . . a Column of Air, of many miles in height, leaning upon some springy Corpuscles of Air here below, may have weight enough to bend their little springs, and keep them bent: As, (to resume our former comparison,) if there were fleeces of Wooll pil'd up to a mountainous height, upon one another, the hairs that compose the lowermost Locks which support the rest, would, by the weight of all the Wool above them, be as well strongly compress'd as if a Man should squeeze them together in his hands, or imploy any such other moderate force to compress them. So that we need not wonder, that upon the taking off the incumbent Air from any parcel of the Atmosphere here below, the Corpuscles, whereof that undermost Air consists, should display themselves, and take up more room than before."

Mariotte (p. 151). "On peut comprendre à peu près cette différence de condensation de l'Air, par l'exemple de plusieurs éponges qu'on auroit entassées les unes sur les autres. Car il est évident, que celles qui seroient tout au haut, auroient leur entenduë naturelle : que celles qui seroient immédiatement au dessous, seroient un peu moins dilatées; et que celles qui seroient au dessous de toutes les autres, seroient très-serrées et condensées. It est encore manifeste, que si on ôtoit toutes celles du dessus, celles du dessous reprendroient leur étenduë naturelle par la vertu de ressort qu'elles ont, et que si on en ôtoit seulement une partie, elles ne reprendroient qu'une partie de leur dilatation."

Those curious in such antiquarian details will probably find a rich reward by making a careful comparison of these two works; and in tracing the connection between the *Liber integer*, and its fons et origo, the paper of Sir Christopher Wren.

Condorcet, in his *Éloge de Mariotte*, says:—"Les lois du choc des corps avaient été trouvées par une métaphysique et par une application d'analyse, nouvelles l'une et l'autre, et si subtiles, que les démonstrations de ces lois ne pouvaient satisfaire que les grands mathématiciens. Mariotte chercha à les rendre, pour ainsi dire, populaires, en les appuyant sur des expériences, &c." *i.e.*, *precisely* what Wren had thoroughly done before him.

"Le discours de Mariotte sur la nature de l'air renferme encore une suite d'expériences intéressantes, et qui étaient absolument neuves." This, as we have seen, is entirely incorrect.

But Condorcet shows an easy way out of all questions of this kind, however delicate, in the words:—"Ou ne doit aux morts que ce qui peut être utile aux vivants, la vérité et la justice. Cependant, lorsqu'il reste encore des amis et des enfants que la vérité peut affliger, les égards deviennent un devoir; mais au tout d'un siècle, la vanité peut seule être blessée de la justice rendue aux morts."

Thus it is seen that even the turn of one of Newton's phrases serves, when rightly viewed, to dissipate a widespread delusion:—and that while Boyle, though perhaps he can scarcely be said to have been "born great," certainly "achieved greatness;" the assumed parent of *La Loi de Mariotte* (otherwise *Mariottesches Gesetz*) has as certainly had "greatness thrust upon" him.

3. The Graphic Analysis of the Kinematics of Rigid-Bar Mechanisms. By Professor R. Smith. Communicated by Professor Tait.

4. Note on the Necessity for a Condensation-Nucleus.

By Professor Tait.

The magnificent researches of Andrews on the Isothermals of Carbonic Acid formed, as it were, a nucleus in a supersaturated solution, round which an immediate crystallization started, and has since been rapidly increasing.

They gave the clue to the explanation of the paradoxical result of Regnault, that hydrogen is less compressible and other gases more compressible, under moderate pressures, than Boyle's Law indicates; and to that of the companion result of Natterer that, at very high pressures, all gases are less compressible than that law requires. Thus they furnished the materials for an immense step in connection with the behaviour of fluids *above* their critical points.

But they threw at least an equal amount of light on the liquid-vapour question—*i.e.*, the behaviour of fluids at temperatures *under* their critical points. In Andrew's experiments there was a commencement, and a completion, of liquefaction; each at a common definite pressure, but of course at very different volumes, for each particular temperature.

In 1871 Professor J. Thomson communicated to the Royal Society a remarkable paper on the *abrupt* change from vapour to liquid, or the opposite, indicated by these experiments. He called special attention to the necessity for a "start," as it were, in order that these changes might be effected. [It is to this point that the present note is mainly directed, but I go on with a brief analysis of

Thomson's work.] He pointed out that there were numerous experiments proving that water could be heated, under certain conditions, far above its boiling point without evaporating; and that, probably, steam might be condensed isothermally to supersaturation without condensing. Hence he was led to suggest an isothermal of continued curvature, instead of the broken line given by Andrews, as representing the *continuous* passage of a fluid from the state of vapour to that of liquid; the whole mass being supposed to be, at each stage of the process, in the same molecular state.

In Clerk-Maxwell's *Treatise on Heat,* this idea of J. Thomson's was developed, in connection with a remarkable speculation of W. Thomson,* on the pressure of vapour as depending on the curvature of the liquid surface in contact with it. This completely accounts for the deposition of vapour when a proper nucleus is present. Maxwell showed that it could also account for the "singing" of a kettle, and for the growth of the larger drops in a cloud at the expense of the smaller ones.

The main objection to J. Thomson's suggested isothermal curve of transition is that, as Maxwell points out, it contains a region in which pressure and volume increase or diminish simultaneously. This necessarily involves instability; inasmuch as, for definite values of pressure at constant temperature within a certain range in which vapour and liquid can be in equilibrium, Thomson's hypothesis leads to three different values of volume: two of which are stable; but the intermediate one essentially unstable. According to Maxwell, the extremities of this triple region correspond to pressures, at which, regarded from the view of steady increase or diminution of pressures, either the vapour condenses suddenly into liquid, or the liquid suddenly bursts into vapour.

If this were the case, no nucleus would be *absolutely* requisite for the formation either of liquid from vapour or of vapour from liquid. All that would be required, in either case, would be the proper increase or diminution of pressure;—temperature being kept unaltered. The latent heat of vapour, which we know to become less as the critical point is gradually arrived at, would thus be given off in the explosive passage from vapour to liquid. It is difficult to see, on this theory, how it can be explosively taken in on the sudden passage from liquid to vapour.

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* *Proc. Roy. Soc. Edin.*, 1870.

Aitken's experiments tend to show, what J. Thomson only speculatively announced, that possibly vapour may not be condensed (in the absence of a nucleus), when compressed isothermally, even at ranges far beyond the *maximum* of pressure indicated in Thomson's figures. Hence it would appear that the range of instability is much less than that given by Thomson's figures, and may (perhaps) be looked on as a vanishing quantity; the corresponding part of the isothermal being a finite line parallel to the axis of pressures, corresponding to the sudden absorption or giving out of latent heat.

Monday, 2nd February 1885.

THOMAS STEVENSON, Esq., Memb. Inst. C.E., President, in the Chair.

The following Communications were read:—

1. The President delivered the following Address:—

With the prospect of addressing you for the first time since you did me the very great honour of asking me to act as your President, I felt somewhat at a loss to determine on what subject I should venture to speak to you; but, at last, I resolved to refer to an exceedingly important question of maritime engineering practice, which gave rise to very lengthened inquiries before Committees during the last two Sessions of Parliament, involving a contemplated expenditure of many millions sterling, and deeply affecting, for good or evil, the entrance from the sea to the great port of Liverpool. I refer to the proposed Manchester Ship Canal.

That scheme (for the following remarks have, of course, no reference whatever to the scheme as now proposed for sanction by Parliament) cannot but be regarded as involving one of the most momentous physical questions that ever was raised in Britain, and although such a question may, perhaps, never again become one of national import in this country, still, I think, no apology is necessary for bringing it before the notice of this Society.

About half a century ago a principle was admitted into the practice of the maritime engineer, which was considered at the time to be sound, and which the majority of the profession until lately regarded of universal application, from its having for many years been practically tested in many different cases with the most gratifying success.

The principle to which I allude is the confining of the ebb and flow of the river and tidal water to one fixed channel of a certain width by means of what are called *training walls.* These are low structures of stone or timber, placed in tidal estuaries, and carried to such a height only as is just sufficient to guide the direction of the first of the flood tide and last of the ebb. It was found, in practice, that a very small elevation of those walls above the bottom was sufficient to restrain and direct the course of the heaviest land floods and strongest tide currents; and this, because such floods and currents always increase in volume more or less slowly, so that if the walls project only high enough to guide the comparatively gentle beginnings of those discharges, they will be sufficient to direct them after they have attained their greatest volume and highest velocity; for, as the flood increases gradually, the depth will in like manner always go on increasing in the line of the directing wall, since the flood must necessarily continue to follow the line of least resistance, which will always be in the line of greatest depth; so that (excepting in very rare cases) all that is needed to control the direction of the heaviest land flood or the highest tide is to fix the direction of the first of either tide or freshet—*obsta principiis* by the time that either flood or tide has submerged the training walls; the line of direction of such flood or tidal current has been already permanently fixed.

Principally, by means of those walls and steam dredging, the river Clyde has been increased from a depth of 13 feet 8 inches to no less than 26 feet up to Glasgow; but the soundness of this system, which was so triumphantly carried out on the Clyde, has also been, as I have said, proved over and over again to be no less applicable to other rivers as well.

The Ribble, the Tyne, the Tees, the Nith, the Lune, &c., though varying in many of their physical characteristics, have all been improved in the same way, without producing any reduction in depth by the silting up of their sea approaches.

This modern mode of improvement was therefore naturally generally regarded as thoroughly safe, and almost free from any suspicion of danger; yet, nevertheless, we have within the last year or two witnessed the greatest suspicion and excitement about the risk of silting in connection with the intended Manchester Ship Canal, as originally proposed to be carried out, by the erection of training walls in the estuary of the river Mersey. But a few months ago very eminent engineers were ranged on opposite sides in Parliamentary Committees; and it was only, after an almost unprecedented struggle that a physical question was decided, which, in the opinion of many, myself among the number, might—settled otherwise than it was—have ended in something very like a national calamity, namely, the silting up of the sea approaches to Liverpool.

After not much short of half a century of the most varied experience in different parts of the United Kingdom, the profession hesitated to answer with a united voice either aye or no, one of the most important questions ever laid before them by the public. Thirty years of practice, and of successful practice, might have been thought enough to give a satisfactory solution; but when the question was laid before us, with regard to this particular river, it took a new shape. New considerations arose, and new criteria were evolved. This was a lesson to me, and, I think, it should be one to the profession. Among those who from the first confidently predicted that disastrous consequences from silting would follow in the Mersey, I may mention the names of Mr Vernon Harcourt, C.E.; Mr Lyster, the engineer of the Liverpool Docks; and Captain Hills, the marine surveyor of the port of Liverpool.

The Mersey was, at first sight, no exceptional river. The rise and maximum velocity of tide was much the same as in the Ribble; the breadth of estuary was similar to that of the Tees; and the matter in suspension was much less than in the Lune. Yet training walls and steam dredging had increased the depth in the Ribble to between 7 and 8 feet up to Preston; in the Lune to 4 feet up to Lancaster; and in the Tees from 3 to 20 feet as high as Stockton.

With such facts before me, I was slow in coming to any decided view. My instincts or "my feelings," as Smeaton would have expressed it, no doubt warned me against the adoption of training walls at the Mersey, but without telling me why. My brother and myself had successfully improved eleven different navigable rivers by means of training walls and dredging. There was not one physical characteristic of the Mersey which, viewed by itself, would render inapplicable the same treatment, and it had not yet occurred to me that certain characteristics, any one of which was harmless in itself, might, when taken together, justify fears of disaster; so that it was not until I had resurveyed three of the rivers I had formerly improved on the system of training walls that I concluded it was solely due to the existence of certain characteristics which existed in all the three rivers, that the deposits of sand had so quickly followed the erection of the training walls. Hence I concluded that it would be unsafe to risk training walls in the Mersey, where similar characteristics were not only more numerous, but much more strongly marked.

The river Lune was the navigation that first startled me. It was improved by training walls and by steam dredging of the channel, with an eye solely to improving the navigation, and with no afterthought of reclaiming land by accretion. The walls were but 3 feet high, and placed strictly parallel to the stream, nor was any part protected, still less enclosed, by cross walls. Two years after the completion of the work, however, it was found that a considerable accretion of sand had taken place. In 1851, being only two years after the works were finished, a marine survey was made, which showed that the amount of accretion behind the walls had already amounted to 3000 cubic yards of sand, although that amount of deposit was quite balanced by a gain of additional water admitted from the sea through the deepened channel. In 1884, however, thirty-four years after the completion of the works, I had the Lune once more surveyed, when it was found that 230 acres were covered with grass, thus establishing the important physical fact that the deposit had risen to the level of high-water neap tides, and the total accretion had reached the amount of 5,536,060 cubic yards. In the same year I had a fresh survey taken of the river Nith below Dumfries,—a navigation which had been improved by training walls alone,—and the accretion was found to amount to 3,192,970 cubic yards; and again, in the same year, I revisited the third river, to which I have referred,—the Ribble below Preston,—and found that about 5000 acres of what was formerly estuary were grassed over.

These three rivers had, therefore, since the completion of the training walls, been undoubtedly subject to large deposits of sand; but the original improvement in depth, and increased velocity in the propagation of the tidal wave, were still maintained, so that, as navigation works, they must be held to have been quite successful. Indeed, if similar improvements had to be done over again, I should not propose to make any change whatever in the mode of carrying out the works. It will be seen, however, from these cases, that training walls exert a double influence—one directly for good, by enlarging and deepening the main channel; another directly for evil, since the deposit of silt behind the walls must reduce the scouring power by diminishing the area filled at each tide, and thus keeping back a corresponding quantity of tide-water from entering the estuary. Thus the ultimate result for good or for evil must be in each case determined by a comparison of the relative amounts of these *plus* and *minus* quantities.

Here, then, we have the first and elementary ground of that negative answer, which I and others of my professional brethren returned to the question proposed for solution in the case of the Mersey.

If, for instance, the waters of a navigation contained no matter in suspension, the system of training walls would give merely a plus result, and their influence be therefore wholly beneficial; but even where there was matter in suspension in the land-locked portion of the estuary,—by which I mean that portion between the training wall and the river bank behind it, where alone siltage can go on, —and supposing the area of that portion should happen to be very small in comparison to the whole extent of the open estuary, the *plus* result would still outweigh the *minus*, and the balance of the influence be for good. What naturally is land-locked might no doubt be silted up, and lost to the navigation; but then, if that extent be small compared with what still remains open to the entrance of the tides, the river on the whole will be improved. If, on the other hand, the land-locked portion of the estuary be very large, the total deposit may not only far more than counterbalance the additional water gained by extra water passing up the improved channel, but leave so little space for the reception of tidal water in the estuary, that the amount of scouring water that can in future come in from the sea will be insufficient any longer to keep open either the bar or the lower channels, then the navigation will be ruined.

This possibility once admitted, that the benefit of the application of training walls really depends on the relative proportion of the good and evil influences, it becomes necessary next to consider in more detail the natural features which I believe tend to the formation and deposit of matter in suspension, and which ought, therefore, to be regarded as dangerous in the application of training walls to any river where there is a wide estuary. These characteristics have, so far as I know, not been before referred to in connection with the subject now under consideration.

In future, then, I consider it essential that in every case the following characteristics ought to be fully inquired into and taken into account before training walls are resorted to:—

I. *A High Rise of Tide.*

Because if there be matter in suspension in the sea (due to the action of the waves on the bottom when they reach shoal water), the higher the rise of tide the greater will be the amount of solid matter that will be brought from the sea.

II. *A Rapid Tidal Flood Current.*

Because, from erosion of the sides and bottom, the more rapid the current the greater the amount of sandy and silty matter that will be disturbed and carried up the river.

III. *Tidal Bore.*

Where there is a tidal bore with a cresting or breaking head, which, from its necessarily acting in shallow water, and with a very high velocity, greatly cuts up the bottom and sides of the channel, so as to set free a large amount of matter.

IV. *A Bar.*

Because if there is either a bar or extensive sandbanks, with heavy breaking waves, at the mouth of a navigation, large quantities of matter are set loose, which may be carried inward by the flood tide, so that a bar and the adjoining sea shores may be regarded as natural *silt producers,* which never fail to generate a constant and large supply of matter for deposit.

V. *Natural Limit to the Erection of Walls.*

Given the above features, the only known expedients for counteracting the evil of the reduced amount of scouring power, consequent on siltage, are those proposed by my brother, Mr D. Stevenson, about forty years ago—first, by continuing the training walls down to the sea, and thus concentrating what yet remains of the reduced scouring power upon the lower channel of the navigation; and, second, by the erection of breakwaters in deep water to shelter the bar from the beating of the sea upon its surface, and thus to neutralise, if not to annihilate, these great originators of silt. If, owing to the local circumstances of the case, both of these plans be impracticable, we may almost regard these circumstances as presenting a fifth and serious element of danger.

VI. *Amount of Matter in Mechanical Suspension.*

But, again, instead of arguing from principles, *the amount of matter in suspension* may be ascertained by direct observation, and will thus form another definite element in the comparison of rivers.

VII. *The Existence of a Wide Land-locked Estuary.*

And last, and one of the most dangerous of all, *the existence of a wide land-locked estuary,* as already referred to.

Such are the criteria which we shall now proceed to apply to the comparison of the *river Mersey* with six rivers which have all been improved by training walls.

In three of these—the *Nith, Lune,* and *Ribble*—there have been, as we have seen, great accretions; in the other three—the *Clyde, Tyne,* and *Tees*—although training walls have been also long since employed, there have been hardly any or no deposit.

Let us look first at the three last cases, as to which it was very strongly and pertinently urged before Parliament that no deposit has ever taken place, although training walls have been in existence for many years.

Now it is not surprising that the introduction of training walls has not produced deposit in all rivers, for the fact seems just as certain, on physical grounds, that they will not produce accretion in some rivers as that they will produce it in others. It is essential that before accretion can, in any case, take place there must be matter in suspension, or, in other words, there must in some part of the river be water in such rapid motion as to break up and move the materials of the bottom and sides of the channel, while at some other part of the river there must be such retardation of the velocity of the water as to cause the deposit of the matter in suspension; and, besides this, there must be a large receptacle to contain this matter, and sufficiently sheltered from waves and currents to retain it after deposit.

The Clyde.—Having for the last twelve years acted as engineer to the Clyde Lighthouses Trustees, who are the Conservators of the lower estuary of that river, and having during that time been engaged in conducting very extensive dredging operations, I am necessarily fully acquainted with its peculiarities, and can state that there is only one point of resemblance of any consequence between that river and the Mersey, viz., that the lower land-locked estuary of the Clyde is about the same in width as that of the widest part of the Mersey below Halehead. But then there is no training wall *connected with the shore* for guiding the channel through this wide estuary, so that we can get no information on the subject at issue. The walls which do exist in the Clyde are mainly in the upper part of the river, where the navigable channel is only about 400 feet wide, and where there is no large space left for silting up to take place between these walls and the land.

With the one exception of the wide estuary to which I have referred, none of the characteristics which are favourable to deposit exist in the Clyde, as I will now show. The rise of tide in the Clyde is only 10½ feet, as against 30 feet at the Mersey. The maximum velocity of the tidal flood current is only about half a mile per hour, as against 7 at the Mersey. There is no tidal bore at the Clyde, and a very rapid one at the Mersey. There is neither a bar nor any heavy breaking waves at the Clyde, but there are both at the Mersey. There is only 1 cubic inch of silt to 1 cubic yard of water at the Clyde, and 33 cubic inches to 1 cubic yard at the Mersey.

There is a depth of 20 fathoms at low water where the Clyde joins the sea, which is land-locked and sheltered, while there is only 1½ fathom at low water on the bar at the Mersey, which is in the open and stormy Irish Channel.

In short, no two sandy rivers can well be more different in their main features than the Mersey and the Clyde. It is therefore quite what might be expected, that there should be scarcely any accretion in the Clyde.

The Tyne.—At the Tyne matters are, on the whole, in much the same state as at the Clyde, for the rise of tide is only 13 feet. There is no bore, and the bar has been deepened and sheltered by the extensive breakwaters now in progress; and though there are training walls, they have been erected so close to the land as to leave no large receptacle for silt to be deposited, so that there is hardly any similarity between the Tyne and the Mersey.

The Tees.—As at the Tyne, the best means have in the case of the Tees been already employed of counteracting the risk of silting by the extension of the training walls down to the sea, and the erection of sheltering breakwaters which protect the bar, where there used to be only 3 feet of depth, whereas there is now a depth of 20 feet; and, besides, there is only 15 feet of a rise of tide, with a flood tide current of only 4½ miles an hour, so that no argument applicable to the Mersey can be derived from the Tees, any more than from the Clyde or the Tyne.

From these rivers we turn to the facts relating to those in which silting has really taken place, and where training walls have been adopted, but where there are dangerous characteristics, but those of a comparatively subdued nature compared with those which are in the Mersey.

The results of the statements already made may be thus shortly summarised. That silting up to grass level may take place in an estuary, though the rise of tide may be no greater than 24 feet, as at the Ribble; where there is no bore, as at the Lune and Ribble; where there is no bar, as at the Nith; and where the ratio of matter in mechanical suspension may be as low as 3·68 cubic inches, as at the Nith.

It is obvious, therefore, that there is every possible reason to expect that if walls be erected accretion will take place up to the grass level in the Mersey, where we find combined every one of these characteristics of danger, which is not the case in any of the other rivers referred to. Not only so, but in every case those characteristics are more strongly marked. To all this must be added the all-important fact that if silting take place, as must be expected, there seems no practical mode of remedying the evil; for, owing to local circumstances, the walls cannot be extended so as to shelter the bar, as that would require no fewer than ten miles of breakwater to be erected in the stormy waters of Liverpool Bay.

But while the erection of training walls on the Nith, Lune, and Ribble has thus been followed by large deposits of silt, it is still, and with truth, contended that these navigations have been, upon the whole, greatly improved.

We are here thrown back once more upon the question of ratio between the effects for good and evil; of ratio between the extra amount of tidal water gained by deepening and enlarging the mid-channel, and the amount of scouring water altogether excluded by accretion in the land-locked estuary, on which ratio, as already stated, the whole question mainly depends.

But how is the value of this ratio to be determined, and what limit is it safe to observe?

I can only here refer to the case of another river, not yet alluded to, viz., the *Tay*, which, during the years 1833 to 1844, was improved according to designs of my late father, so that an additional depth of 4 feet and a tidal acceleration of 50 minutes were got up to the quays at Perth. But no training walls were ever proposed to be erected in the lower and wider part of the estuary, where there is sand and silt.

In the year 1845, however, certain riparian proprietors proposed to embank land in the lower and wider estuary, and the authorities of the important harbour and docks of Dundee became alarmed for the safety of their interests, and applied to the Admiralty to send an engineer of eminence to report on the whole subject. The engineer selected was the late Mr James Walker, the then permanent President of the Institution of Civil Engineers, and the acknowledged head of the profession in all maritime matters. In an elaborate report he condemned the proposed works, on the ground that they would exclude *one-twentieth* of the scouring tidal power of the Tay.

In the opinion, then, of Mr Walker, the first engineer of his day, and the one who carried out most of the later improvements of the river Clyde between Glasgow and Dumbarton, the loss of *one-twentieth* of the natural tidal scouring power at the Tay ought to be firmly resisted. But is this caution to be accepted, or should it be regarded as excessive?

Turning to ascertained fact, the only one to which I can appeal is that observed at the Lune, for I have no means of knowing what has actually taken place at either the Nith or the Ribble.

The loss of tidal scouring water due to the training walls at the Lune was found to be *one-tenth*, but from this loss no evil consequences appear to have resulted. We have then, by way of criteria, only two to guide us, viz., Mr Walker's opinion that a loss of *one-twentieth* would prove injurious at the Tay, and the ascertained fact that a loss in the case of the Lune of *one-tenth* of the scouring power did no harm.

Turning now to the case of the Mersey, and assuming, as we have more than sufficient reason for doing, that deposit of sand will take place up to the grass level in the land-locked estuary of that river, as it has done already in the kindred but less dangerous cases referred to, we have next to ascertain what would be the ratio of loss in that case.

The total amount of deposit, as ascertained from actual soundings, would be about 460,000,000 cubic yards, measuring from the present bottom up to grass level, while the additional tidal capacity which will be simultaneously gained through dredging and scouring the improved channel will amount to about 14,000,000 cubic yards; and, deducting the one from the other, we find a balance of loss of 446,000,000 cubic yards.

But in order to judge of the effect of this loss, we must contrast it with the total amount of tidal water above the level of low-water spring tides, which at present enters the estuary from the sea at the mouth of the river, and which fills the tidal basin up to Runcorn. This amounts to about 733,000,000 cubic yards, to which must be added the average fresh-water drainage of the Irwell and Mersey districts, which I compute at about 9,000,000 cubic yards in twelve hours, making a total scouring power of 742,000,000 yards.

Compare this with the resulting loss, and we find that not *one-twentieth*, which Mr Walker feared to authorise at the Tay—not *one-tenth*, as has actually taken place at the Lune—but *one-half of the whole scouring power would be excluded from the estuary of the Mersey.*

With such a result as this, I cannot for a moment doubt that the promoters of the Manchester Canal, and Mr E. Leader Williams, their engineer, before going to Parliament for the present Bill, exercised a wise discretion in resolving to adopt the suggestion of Mr Lyster of Liverpool to keep the new Ship Canal—*i.e.*, the one now before Parliament—altogether outside of, and therefore clear of the land-locked estuary of the Mersey, so as no longer to expose the works to the rapid tides and land floods, as in the first scheme.

The lesson, then, as it seems to me, which is to be derived from all that has been said, is simply that where there is a conjunction of an extensive land-locked estuary, with all, or with many of those conditions favourable to silting which I have defined, such a state of matters should in all cases be regarded as more or less dangerous, and especially so in any river where the depth is already reduced by the existence of a shallow bar lying beyond the mouth of the river, and exposed to a heavy breaking sea, from the action of which it cannot be protected by breakwaters.

2. On Evaporation and Condensation. By Professor Tait.

(*Abstract.*)

While I was communicating my Note on the *Necessity for a Condensation Nucleus* at the last meeting of the Society, an idea occurred to me which germinated (on my way home) to such an extent that I sent it off by letter to Professor J. Thomson that same night.

J. Thomson's idea, which I had been discussing, was to preserve, if possible, physical (as well as geometrical) *continuity* in the isothermal of the liquid-vapour state, by keeping the *whole* mass of fluid in one state throughout. He secured geometrical, but not physical, continuity. For, as Clerk-Maxwell showed, one part of his curve makes pressure and volume increase simultaneously, a condition essentially unstable. The idea which occurred to me was, while preserving geometrical continuity, to get rid of the region of physical instability, *not* (as I had suggested in my former Note) by retaining Thomson's proposed finite maximum and minimum of pressure in the isothermal, while bringing them infinitely close together so far as volume is concerned, and thus restricting the unstable part of the isothermal to a finite line parallel to the pressure axis; but, *by making both the maximum and minimum infinite.* Geometrical continuity, of course, exists across an asymptote parallel to the axis of pressures; so that, from this point of view there is nothing to object to. On the other hand there is essentially physical discontinuity, in the form of an impassable barrier between the vaporous and liquid states, so long at least as the substance is considered as homogeneous throughout.

It appeared to me that here lies the true solution of the difficulty. As we are dealing with a fluid mass essentially homogeneous throughout, it is clear that we are not concerned with cases in which there is a molecular surface-film.

Suppose, then, a fluid mass, somehow maintained at a constant temperature (lower than its critical point), and so extensive that its boundaries may be regarded as everywhere infinitely distant, what will be the form of its isothermal in terms of pressure and volume?

Two prominent experimental facts help us to an answer.

First. We know that the interior of a mass of liquid mercury can be subjected to hydrostatic *tension* of considerable amount without rupture. The isothermal must, in this case, *cross* the line of volumes;—and the limit of the tension would, in ordinary language, be called the cohesion of the liquid. I am not aware that this result has been obtained with water free from air; but possibly the experiment has not been satisfactorily made. The common experiment in which a rough measure is obtained of the force necessary to tear a glass plate from the surface of water is vitiated by the instability of the concave molecular film formed.

Second. Aitken has asserted, as a conclusion from the results of direct experiment, that even immensely supersaturated aqueous vapour will not condense without the presence of a nucleus. This may be a solid body of finite size, a drop of water, or fine dust-particles.

Both of these facts fit perfectly in to the hypothesis that the isothermal in question has an asymptote parallel to the axis of pressure; the vapour requiring (in the absence of a nucleus) practically infinite pressure to reduce it, without change of state or of temperature, to a certain finite volume; while the liquid, also without change of state or temperature, may by sufficient hydrostatic *tension* be made to expand almost to the same limit of volume.

This limiting volume depends, of course, on the temperature of the isothermal; rising with it up to the critical point.

The physical, not geometrical, discontinuity is of course to be attributed to the latent heat of vaporisation. The study of the adiabatics, as modified by this hypothesis, gives rise to some curious results.

It is clear that the experimental realisation of the parts of the here suggested curve near to the asymptote, on either side, will be a matter of great difficulty for any substance. But valuable information may perhaps be obtained from the indications of a sensitive thermo-electric junction immersed in mercury at the top of a column which does not descend in a barometer tube of considerably more than 30 inches long, when the tube is suddenly placed at a large angle with the vertical; or from those of a similar junction immersed in water, when it has a concave surface of great curvature from which the atmospheric pressure is removed.

Nothing of what is said above will necessarily apply when we have vapour and liquid in presence of one another, or when we consider a small portion of either in the immediate neighbourhood of another body. For then we are dealing with a state of stress which cannot, like hydrostatic pressure or tension, be characterised (so far as we know) by a single number. The stress in these molecular films is probably one of tension in all directions parallel to the film, and of pressure in a direction perpendicular to it. Thus it is impossible to represent such a state properly on the ordinary indicator-diagram. This question is still further complicated by the possibility that the difference between the internal pressures, in a liquid and its vapour in thermal equilibrium, may be a very large quantity.

3. Note on Ectocarpus. By John Rattray, B.Sc. Communicated by John Murray, Esq.

4. Some remarkable Concretions, collected in the neighbourhood of Philadelphia by the Rev. J. M. Macdonald, were exhibited.

PRIVATE BUSINESS.

The following Candidates were balloted for, and declared duly elected Fellows of the Society:—Professor W. R. Hodgkinson; Mr Hugh Robert Mill, B.Sc.; Mr John Rattray, M.A., B.Sc.; Mr William Miller, S.S.C.; Dr Alfred Daniell, M.A.; and Captain W. de Wiveleslie Abney, F.R.S.

Monday, 16*th February* 1885.

JOHN MURRAY, Esq., Ph.D., Vice-President,
in the Chair.

The following Communications were read:—

1. Preliminary Report on the Cephalopoda collected during the Cruise of H.M.S. "Challenger." Part I. The Octopoda. By William E. Hoyle, M.A. (Oxon.), M.R.C.S.

(*Published by permission of the Lords Commissioners of the Treasury.*)

The collection of Cephalopoda made by H.M.S. "Challenger," though not extensive, is of great interest, not only because a considerable number of new species were discovered, but also because several rare forms were found in fresh localities, and because a few specimens (many of them fragmentary) were obtained, which bear interesting relations to those already known.

The following synopsis will indicate the general nature of the collection:—

Genera.	Previously known species.	New species.
Octopus, Lamk., . . .	7	11
Alloposus, Vll., . . .	1	...
Eledone, Leach, . . .	1	2
Japetella, Hoyle, . . .	...	2
Cirroteuthis, Eschr., . .	...	3
Amphitretus, Hoyle, . .	...	1
Tremoctopus, d. Ch. . .	2	...
Argonauta, Linn. . . .	1	...

The distribution of the species obtained furnishes an important instance of the general rule that while pelagic forms belong to but few species, each of which has a wide range of distribution, littoral genera are represented by very many species each confined within a narrow area. In the case of the Cephalopoda this law has been expressed by Professor Steenstrup in his division of both Octopoda and Decapoda into two groups, *littorales* and *pelagici*;* to the former of these belongs the genus *Octopus* of which a distinct type was furnished by almost every resting-place of the expedition. Out of about eighteen species collected sixteen came not from dredging stations, but from shore collections; and of those obtained by the dredge or trawl only two were found in depths exceeding 500 fathoms.

The genus *Eledone* furnished two new species; one characterised by a short stumpy contour, and the other interesting as purporting to come from a great depth (1950 fathoms).

The most striking novelties of the Octopoda are to be found among the pelagic forms. The genus *Cirroteuthis*, hitherto known only from the coast of Greenland, has been found also in the Southern and Pacific Oceans; and the specimen from the former locality is remarkable for its size, which exceeds one yard in length.

Allied to *Cirroteuthis* is the new genus *Amphitretus*, which possesses a modification, unique among Cephalopoda, in the fusion of the mantle with the siphon, so as to leave two openings into the branchial cavity.

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* Oversigt k. dansk. Vid. Selsk. Forhandl., p. 69, 1861.

Some fragments which were picked up on the surface of the North Atlantic are beyond all reasonable doubt referable to the curious *Alloposus mollis*, Verrill.

The two specimens which for the present constitute the new genus *Japetella* are of great interest, and may be described shortly as a kind of pelagic *Eledone;* but one of them, *J. diaphana*, shows characters which approximate it to the Cranchiæformes, and it seems quite possible that it will ultimately prove not to be congeneric with *J. prismatica.*

Subjoined are diagnoses of the new forms of Octopoda.

OCTOPUS, Lamarck.

Octopus verrucosus, Hoyle.

Octopus verrucosus, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 222, 1885.

The *Body* is rounded, but so distorted by compression that no further details can be given. The *mantle-opening* extends fully halfway round the body, terminating a little below and behind the eye. The *siphon* is long, evenly conical, and pointed, and extends nearly halfway to the umbrella-margin.

The *Head* is short, not so broad as the body, and with *eyes* but slightly prominent.

The *Arms* are unequal in length, the second pair being considerably the longest, and almost six times as long as the body; they are comparatively stout, and taper gradually. The *umbrella* extends about one-fifth up the longest arms. The *suckers* are fairly close, deeply cupped and marked with radial grooves, between which are numerous very minute papillæ. About four suckers on each lateral arm opposite the umbrella-margin are larger than the others; beyond these they gradually diminish. The *hectocotylus* is present in both specimens, and is very minute (about 2 mm. long in the larger); it is acutely pointed, and the median groove has three transverse ridges.

The *Surface* of the back, dorsal surface of head, and umbrella is covered with irregular closely-set warts, which attain a maximum diameter of several millimetres in the nuchal region; the warts extend on to the ventral surface of the body, where they become much smaller, more even, and average less than 1 mm. in diameter. Above each eye there seems to have been a short cirrus, but these have been rather damaged.

The *Colour* is a dull purplish-grey, very dark above, much lighter below.

Habitat.—Inaccessible Island, Tristan da Cunha. 2 specimens, ♂.

Octopus Boscii (Lesueur), var. *pallida.*

Octopus Boscii (Lesueur), var. *pallida*, Hoyle, *Ann. and Mag. Nat. Hist.* ser. 5, vol. xv. p. 223, 1885.

The *Body* is evenly rounded, with a slight depression in the median ventral line. The *mantle-opening* extends less than halfway round the body, terminating immediately below the eye, and further from it than from the base of the *siphon*, which is long and pointed, has rather a small opening, and extends two-thirds the distance to the umbrella-margin.

The *Head* is short, and not so broad as the body; the *eyes* are only slightly prominent.

The *Arms* are subequal, nearly four times the length of the body, and taper evenly to fine points. The *umbrella* extends one-third up the arms, and is a little wider laterally than dorsally. The *suckers* are closely set, deeply cupped, and marked with regular radial grooves; their biserial arrangement commences immediately after the first. One arm bears a supernumerary sucker in the larger specimen. The *hectocotylus* is of the usual shape and of medium length; it bears fourteen transverse ridges, each subdivided into four minute papillæ. The *circumoral lip* is low and narrow.

The *Surface* is covered with warts, which are largest and most numerous on the dorsal surface of the body, head, and umbrella, and dorsal aspects of the arms, where they have a quadrifid or quinquefid form, usually with a small wartlet in the centre, each forming a figure like a star or rosette. Towards the ventral surface and on the sides of the arms the warts are simple, and much smaller. On the back are about ten long cirri, which are rough with small warts, and above each eye is a very large arborescent cirrus with six or seven smaller ones beside it.

The *Colour* is a pale purplish-grey, shading off to a creamy white on the ventral surface.

Habitat.—Off East Moncœur Island, Bass Strait; 38 fathoms (Station 162). 1 specimen, ♀.

Off Twofold Bay, Australia; 150 fathoms (Station 163 A). 2 specimens, 1 ♂, 1 *juv.*

Octopus australis, Hoyle.

Octopus australis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 224, 1885.

The *Body* is rounded, and wider behind than in front; depressed, and with a well-marked ventral median groove. A sharp narrow ridge extends along either side of the body to the posterior extremity.* The *mantle-opening* extends nearly half round the body. The *siphon* is of medium size, at first the lateral margins are parallel and then tapering rapidly to a blunt point; it extends less than halfway to the umbrella margin.

The *Head* is narrower than the body, and the *eyes* somewhat prominent, dorsally rather than laterally.

The *Arms* are unequal, the lateral being slightly the largest, and about three times as long as the body; they are slender and tapering. The *umbrella* is longer than the length of the body; larger ventrally than dorsally, and larger laterally than ventrally. The *suckers* are prominent and closely set; they are altogether larger on the lateral arms, and extend in a double row to the centre; the radial grooves are deep, and extend quite to the margins. The *hectocotylus* is absent.

The *Surface* of the back of the body, head, and dorsal aspects of the umbrella and arms is covered with thick-set hemispherical pimples, which are also found on the inner side of the membrane between the two dorsal arms, and on the inner surfaces of the arms between the suckers. They are smaller and more sparse on the ventral surface of the body. A large rough cirrus and a few larger pimples are found over each eye. A raised ridge passes backwards from the base of the siphon along the ventro-lateral margin of the body, meeting its fellow of the opposite side at the posterior extremity.*

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* I have considerable doubt whether this be really one of the specific characters; but as it occurs in both specimens, I have thought it well to mention it

The *Colour* is deep purplish on the back, mottled on the sides, and cream below.

Habitat.—Port Jackson, Australia, 6–15 fathoms. 2 specimens, 1 ♀, 1 *juv.*

Octopus hongkongensis, Steenstrup, MS.

Octopus hongkongensis, Steenstrup, MS., in Copenhagen Museum.
„ „ Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 224, 1885.

The *Body* is rounded with a median ventral furrow, and depressed (? from pressure). The *mantle-opening* is slightly less than half the circumference, and terminates midway between the siphon and the eye, and a little behind the latter. The *siphon* is comparatively short and conical, and extends one-third the distance to the umbrella-margin.

The *Head* is comparatively broad, almost as broad as the body, and the *eyes* appear to have been prominent.

The *Arms* are unequal, the ventral considerably shorter than the dorsal and dorso-lateral, on an average six times as long as the body, stout and tapering very rapidly towards the ends. The *umbrella* reaches up one-fifth of the length of the dorsal arms, and is a little wider between the lateral and a little narrower between the ventral arms. The *suckers* are not closely packed, and not enlarged on the dorsal arms, notwithstanding the sex of the specimen. The *hectocotylised* arm is very short, but the modified extremity is long and narrow, with parallel sides tapering only at the extremity to a blunt point; a narrow fillet runs between the two marginal ridges.

The *Surface* of the body is sprinkled dorsally with minute hemispherical warts, which become smaller and gradually disappear on the sides and lower surface; they are also found on the dorsal surface of the head and of the umbrella. Above each pupil stands a small cirrus, immediately behind which is a larger one.

The *Colour* is a dull red on the dorsal surface, lighter and brighter below, sprinkled with dull reddish-brown dots.

Habitat.—The *Hyalonema*-ground south of Japan, 345 fathoms (Station 232). 1 specimen, ♂.

Octopus tonganus, Hoyle.

Octopus tonganus, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 225, 1885.

The *Body* is rounded, depressed, and broader than long, with a marked but shallow median groove on the ventral surface. *Mantle-opening* extends about one-third round the circumference of the body, terminating nearer to the funnel than to the eye. The *siphon* is short and conical, and extends rather more than halfway to the umbrella margin.

The *Head* is small and the *eyes* prominent.

The *Arms* are unequal, the order being 3, 2, 4, 1; on an average they are nearly ten times as long as the body, and taper gradually to very fine points. The *umbrella* is very small, and slightly narrower dorsally than laterally. The *suckers* are for the most part small and closely packed; the first four are arranged in a single row; in the male there are four large ones on each lateral arm opposite the margin of the umbrella, beyond which they gradually diminish. The *hectocotylus* is very minute.

The *Surface* is in general smooth; the back bears a few small papillæ, but owing to the compression of the specimens it is impossible to make out their exact number. There are three minute cirri over each eye.

The *Colour* is on the whole grey, paler below; this is due to dark specks sprinkled more or less closely over a cream-coloured ground. One specimen has a purplish patch at either side of the mantle-opening.

Habitat.—The reefs, Tongatabu. 3 mutilated specimens, 1 ♂, 2 ♀.

Octopus vitiensis, Hoyle.

Octopus vitiensis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser 5, vol. xv. p. 226, 1885.

The *Body* is nearly oblong, but becomes somewhat narrower posteriorly. The *mantle-opening* extends nearly one-third round the body, and terminates some distance below and behind the eye. The *siphon* is long and acutely pointed, and extends about halfway to the umbrella-margin.

The *Head* is broader than the body, with large laterally prominent *eyes*.

The *Arms* are subequal, the two lateral pairs being a little longer than the others ; on an average they are nearly three times as long as the body, and taper rather rapidly about the middle of their length and then evenly to fine points. The *umbrella* extends nearly one-third up the arms, least along the dorsal pair. The *suckers* are sunken, comparatively large, with a dark margin and very well-marked radial grooves. The first two in each arm are in a single row, owing to compression of the arms laterally ; there are no enlarged suckers on the lateral arms. The *hectocotylus* is not developed.

The *Surface* of the dorsum of the body bears minute warts scattered here and there ; over each eye there is a rather large branched cirrus, with a few small ones scattered round it. The internal surfaces of the arms are covered with minute hemispherical warts, so as to resemble shagreen.

The *Colour* is very dark grey, almost black on the dorsal surface and outside the arms ; paler grey below and on the inner surfaces of the arms. The dark covering of the body is not continued over the inner surface of the membrane covering the two dorsal arms.

Habitat.—The reefs, Kandavu, Fiji. 1 specimen, ♀.

Octopus duplex, Hoyle.

Octopus duplex, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 226, 1885.

The *Body* is short and evenly rounded, with the merest trace of a median ventral groove. The *mantle-opening* extends about half round the circumference, and terminates halfway between the siphon and the eye. The *siphon* is relatively long, conical and pointed, and extends about halfway to the umbrella-margin.

The *Head* is small, and the *eyes* rather prominent.

The *Arms* seem to have been unequal, the lateral the longest, but so many have been mutilated that it is difficult to be certain ; they are about four times the length of the body. The *umbrella* extends about one-third up the arms, the furthest between the lateral pairs. The *suckers* are large, close, and prominent, with a narrow margin marked off from the basal portion ; the radial grooves extend to the margin and form notches in it. The *hectocotylus* is not present ; the third right arm has been mutilated, but the stump shows no groove running up the outer ventral margin.

The *Surface* is smooth; an interrupted ridge starting from the base of the siphon passes backwards along the ventro-lateral margin of the body, probably due to contraction. There are three small papillæ above each eye.

The *Colour* is a pale bluish-grey above, shading into cream below.

Habitat.—Off Twofold Bay, Australia, 150 fathoms. (Station 163 A). 2 specimens, 1 ♀, 1 *juv.*

Octopus bandensis, Hoyle.

Octopus bandensis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 227, 1885.

The *Body* is rounded and depressed (? from pressure), and with a slight ventral groove. The *mantle-opening* terminates immediately below and behind the eye. The *siphon* extends as far as the umbrella-margin, and is acutely pointed.

The *Head* is broader than the body, and flattened by compression; the *eyes* are very prominent.

The *Arms* are unequal, the third pair being much the longest and stoutest; on the average they are four times as long as the body; they taper gradually at first and then more rapidly. The *umbrella* is very small. The *suckers* are prominent, the first four being disposed in a single row; and none are enlarged on the lateral arms. The *hectocotylus* is not developed.

The *Surface* is smooth in general, but there are about twelve warts on the back and sides of the body, and a large cirrus over each eye, with several small ones near it.

The *Colour* is deep black, apparently owing to treatment with osmic acid.

Habitat.—Banda. I specimen, *juv.*

Octopus marmoratus, Hoyle.

Octopus marmoratus, Hoyle, *Ann. and mag. Nat. Hist.*, ser. 5, vol. xv. p. 227, 1885.

The *Body* is round, not depressed and a little longer than wide. The *mantle-opening* extends somewhat less than halfway round the body, terminating nearer to the siphon than to the eye, and considerably behind the latter. The *siphon* is small and acutely conical, and extends about one-third the distance to the umbrella-margin.

The *Head* is narrow, and the *eyes* are prominent, where they have not suffered from compression.

The *Arms* are subequal, eight times as long as the body; they are very long and slender, the last property being more marked in the females than in the male; they taper more rapidly at first than near the extremities, which are much attenuated, where it extends almost one-third up the arms; in the females its extent is only one-sixth. The *umbrella* is very wide, especially in the male. The *suckers* are rather large, and closely set; in the male a few suckers opposite the umbrella-margin are slightly, but not markedly, larger than the others. The *hectocotylus* is small, and has about ten small transverse ridges.

The *Surface* appears to have been smooth, except for a few short ridges placed longitudinally on the back and sides; but the skin is shrivelled by the action of the spirit, so that it is difficult to be certain. A conical cirrus is situated above and slightly behind each eye; but in some cases this has been destroyed.

The *Colour* is a stone-grey, with dark pigment disposed in veins like those of marble on the dorsal surface of the body, head, and umbrella; the male is much darker, so that the marbling is almost concealed. Traces of an oval spot are seen in front of and below the eye on both sides of one female specimen and on one side of the other; but this spot is concealed by the dark colouring in the male even if it exist.

Habitat.—On the reefs, Honolulu, Sandwich Islands. 3 specimens, 1 ♂, 2 ♀.

Octopus bermudensis, Hoyle.

Octopus bermudensis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 228, 1885.

The *Body* is spheroidal, acuminate behind, and with a median groove ventrally. The *mantle-opening* extends rather less than half round the circumference of the body, and terminates some distance behind and a little below the eye. The *siphon* is long and smooth, and attached by nearly all its length to the head; it extends fully halfway to the umbrella-margin.

The *Head* is much narrower, and more depressed than the body; the *eyes* are scarcely at all prominent.

The *Arms* are unequal, in the order 1, 2, 3, 4; about six times as long as the body; they are very long and slender, tapering but slightly. The *umbrella* is small. The *suckers* are small, prominent and closely set, and the first four stand in a slightly zigzag line. The *hectocotylus* is absent.

The *Surface* is smooth for the most part, but the skin is wrinkled over the posterior acuminate extremity, owing to the action of the spirit; there is one very small wart over each eye.

The *Colour* is yellow ochre, with two pale sienna patches on the back and on the head.

Habitat.—Bermuda. 1 specimen, ♀ *juv.*

Octopus levis, Hoyle.

Octopus levis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 229, 1885.

The *Body* is oblong, depressed, and bulges a little at the sides. The *mantle-opening* extends about one-third round the circumference, terminating about midway between the siphon and the eye. The *siphon* is short and small, extending scarcely one-third the distance to the umbrella-margin.

The *Head* is almost as broad as the body; and the *eyes* are large, spheroidal, and prominent, with very small circular apertures.

The *Arms* are subequal and short, compared with the body, being about three times its length; they taper gradually to moderately fine points. The *umbrella* is large, extending about one-third of the arms. The *suckers* are small and prominent, and arranged in two rows from the commencement; a narrow well-marked groove runs across the arm between each two suckers (possibly due to contraction). The *hectocotylus* is well-developed, short, and tapering rapidly to a blunt point; the median groove has about ten transverse bars. The *circumoral lip* is unusually thick.

The *Surface* appears to have been perfectly smooth, but is now covered with wrinkles, due to the action of the spirit.

The *Colour* is a dull grey, inclining to stone-colour below.

Habitat.—Off Heard Island, Southern Ocean; 75 fathoms (Station 151). 4 specimens, 1 ♂, 1 ♂ *juv.*, 2 ♀ *juv.*

Octopus januarii, Steenstrup, MS.

Octopus januarii, Steenstrup, MS., in Copenhagen Museum.
,, ,, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 229, 1885.

The *Body* is rounded, widening a little posteriorly; the ventral groove is very faint. The *mantle-opening* terminates just below the eyes. The *siphon* is bluntly conical, and extensively attached to the umbrella; it extends less than halfway to the umbrella-margin.

The *Head* is small, and the sides are entirely occupied by the enormous *eyes*, which are swollen and globular, but with very small palpebral openings. The skin covering them is so thin that the dark pigment within is distinctly visible.

The *Arms* are unequal, the dorsal pair being the largest; on an average they are four times as long as the body. The *umbrella* is about equally wide all round, except that it is a little shorter between the two ventral arms. The *suckers* are comparatively small, prominent, and separate; more widely in one specimen than in the other, probably owing to its being less contracted by spirit. None of the suckers on the lateral arms are enlarged, notwithstanding the sex. The *hectocotylus* is well-developed; it is broad, and tapers rapidly to an acute point; in one specimen about seven transverse ridges can be counted in the proximal half of the median groove; in the distal half and in the other specimen they are indistinct.

The *Surface* is perfectly smooth all over.

The *Colour* is a pale purplish-pink; deeper above than below.

Habitat.—Off Barra Grande (Station 122), 1 specimen, ♂; (Station 137), 1 specimen, .

Eledone, Leach.

Eledone rotunda, Hoyle.

Eledone rotunda, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 230, 1885.

The *Body* is spheroidal, very little longer than broad, and the ventral groove is not marked. The *mantle-opening* extends one-third round the body, ending some distance directly below the eyes. The *siphon* is slightly tapering, and extends one-third the distance to the umbrella-margin.

The *Head* is short, nearly as broad as the body, and the *eyes* are round and rather prominent.

The *Arms* are equal, and about twice as long as the body; they are very stout, and taper gradually to blunt points; their section shows a triangle projecting inwards, and a rounded surface looking outwards; the former much more prominent than the latter. The *umbrella* is wide, extending one-third up the arms, a little further dorsally than ventrally. The *suckers* are comparatively small, closely set, and deeply cupped. There is no trace of a *hectocotylus.*

The *Surface* is perfectly smooth.

The *Colour* is dull purple, palest on the body, and deepest on the inner surface of the umbrella.

Habitat.—The Southern Ocean (Station 137); 1950 fathoms. South Pacific; 2225 fathoms (Station 298). 1 specimen, ♀; 1 specimen, arms only.

Eledone brevis, Hoyle.

Eledone brevis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 230, 1885.

The *Body* is short, rounded, and depressed; blunt behind, and deeply grooved below. The *mantle-opening* extends one-third round the animal, and terminates immediately below the eyes. The *siphon* is short, acutely pointed, and extends less than halfway to the umbrella-margin.

The *Head* is short, and nearly as broad as the body. The *eyes* are spheroidal, and very prominent.

The *Arms* are equal, and about half as long again as the body; they are short, stout, and taper gradually to blunt points. The *umbrella* extends halfway up the arms,—rather more in the largest specimen. The *suckers* are round, prominent, and deeply cupped, and there are about 30 on each arm. The *hectocotylus* is not developed.

The *Surface* is smooth, with the exception of three cirri arranged in a triangle over each eye. Behind the left eye in the largest specimen the skin is elevated into a number of small papillæ, and a few similar ones are seen on the back of the medium-sized specimen.

The *Colour* is a dull purplish-grey above, inclining to pale ochre below.

Habitat.—South Atlantic, 600 fathoms (Station 320). 3 specimens, ♀

JAPETELLA, Hoyle.*

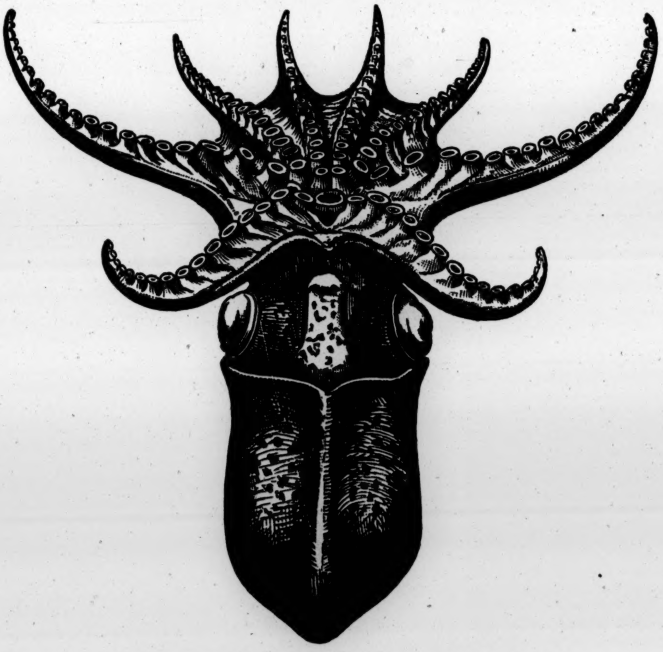
The *Body* is gelatinous in consistency and semitransparent, and more or less oblong in form. The *mantle-opening* is very wide.

The *Arms* are unequal, and the longest only about equal in length to the body. The *umbrella* is small, and the *suckers* arranged in a single row.

Japetella prismatica, Hoyle.

Japetella prismatica, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 231, 1885.

The *Body* is of gelatinous consistency and semitransparent; it is somewhat longer than broad, flat on the dorsum, and with the median ventral region raised into a prominent ridge, which gives the body a prismatic form. The *mantle-opening* appears to have been wide, and to have terminated behind the eyes; but as it has been torn away from the head dorsally, it is impossible to be certain of this. The



Japetella prismatica, Hoyle (natural size.)

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* Named in honour of Professor Japetus Steenstrup.

siphon extends almost to the margin of the umbrella, and is but slightly conical, with a broad truncated extremity. There is no *median septum* in the branchial cavity.

The *Head* is about as broad as the body; the *eyes* are rounded, and prominent laterally, the spherical lens protruding from the middle of each.

The *Arms* are unequal, the third being the largest, one quarter longer than the body, and about one-third longer than the fourth, which is slightly longer than the second, and this than the first, so that the order of length is 3, 4, 2, 1. The arms are stout, and taper gradually to blunt points. The *umbrella* extends about halfway up the dorsal arms and one-fourth up the ventral arms, its extent being intermediate between these extremes in the case of the other arms. The *suckers* are round and prominent, and in many cases show a double margin, due to a thin membrane surrounding the thickened edge of the sucker; they commence one sucker's breadth from the oral margin, and become gradually larger and wider apart as far as the middle of the arm, where they are one sucker's breadth apart, after which they are smaller and closer, and towards the extremity stand in contact. There is no trace of a *hectocotylus*.

The *Surface* of the body appears to have been perfectly smooth; it is covered with torn remains of epithelium, but there are neither warts nor cirri.

The *Colour* is a pale yellowish-grey, and there are numerous reddish-brown chromatophores.

Habitat.—Off the Rio San Francisco, Brazil (Station 125), probably surface. 1 mutilated specimen, sex ?

Japetella diaphana, Hoyle.

Japetella diaphana, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 232, 1885.

The *Body* is gelatinous and semitransparent; it is ovoid in form, and considerably longer than broad. The *mantle-opening* is very wide, extending up behind the eyes on either side. The *siphon* extends two-thirds of the distance to the umbrella-margin, and is truncated at the extremity. There is a *median septum* in the branchial cavity.

The *Head* is nearly as broad as the body, and the *eyes* are large and prominent; they consist of a larger darkly pigmented spheroid, from which protrudes the opaque white smaller spherical lens.

The *Arms* are unequal; the longest (the third pair) are almost as long as the body, and are nearly twice as long as the fourth, which are the shortest, the order of length being 3, 2, 1, 4; they taper rapidly to fine points. The *umbrella* is very small, extending about one-fourth up the dorsal and ventral arms, a little further up the dorso-lateral and lateral arms, and being least developed in the space between the ventral and ventro-lateral arms. The *suckers* have assumed, owing to shrinking, a quadrangular or triangular form; they are prominent, and marked by two constrictions, one at the base, separating them from the arm, the other rather more than halfway up. There is no trace of any *hectocotylus*.

The *Surface* appears to have been smooth originally, but the epithelium has been to a great extent stripped off.

The *Colour* is very pale yellowish-grey, marked with numerous longitudinally disposed oblong chromatophores.

Habitat.—North of Papua (Station 220); surface. 1 specimen, sex?

CIRROTEUTHIS, Eschricht.

Cirroteuthis magna, Hoyle.

Cirroteuthis magna, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 233, 1885.

The *Body* is oblong, about twice as long as broad, and rather broader than deep. The *mantle-opening* is circular, but little larger than the base of the siphon, and its margins are continuous with two ridges on the sides of the latter. The *siphon* is conical, and slightly swollen at the tip; it is not connected to the head by ligaments. The *fins* are obovate in form, about four times as long as broad, and thickened along the posterior margin. The *dorsal cartilage* is saddle-shaped, and elongated from side to side, not antero-posteriorly.

The *Head* is directly continuous with, and somewhat narrower than the body; the *eyes* are spheroidal, the lens spherical, and the palpebral opening circular.

The *Arms* are subequal, three and a half times as long as the body; they are slender, and more resemble thickenings of the web than independent arms; they are thickest about two-thirds the distance along them, and terminate in a delicate slender tip, which projects beyond the web. The *umbrella* is a thin delicate membrane, very largely developed, and when fully expanded its diameter was probably nearly three times as large as its depth. It extends from the tip of the ventral arm on either side backwards, and becomes gradually narrower, so that it only extends along the proximal two-thirds of the next arm, and passes beneath it to be attached in a crescentic line to the outer surface of the web, which similarly passes backwards from the tip of this arm; this arrangement is usually described by saying that the arm does not lie in the umbrella for the proximal two-thirds of its length, but is joined to it by a vertical web, but this does not so correctly represent the disposition of the parts. The umbrella stretches across from tip to tip of the two dorsal arms, between the proximal two-thirds of the two ventral arms. The *suckers* commence about 1 cm. from the oral margins, and the first four lie pretty close together within a space of less than 2 cm., after which they gradually become further apart, an interval of 2–3 cm. intervening between each two; about two-thirds along the arm they stand closer together, and are very large, but after this they again become smaller, and stand in close contiguity with each other. The proximal suckers are small, prominent, and rather soft, and seem to contract by folding the lateral margins over towards each other, so as to present the appearance of a half-closed eyelid; but the largest suckers are firm and muscular, and consist of a globular basal portion imbedded in the arm and a short cylindrical projecting portion. The *cirri* commence between the fourth and fifth suckers as very minute prominences, which gradually increase in length until halfway along the arm they attain a maximum length of 8 cm., after which they decrease rapidly, and cease opposite the attachment of the web to the ventral aspect of the arm.

The *Surface* of the body has been entirely denuded of epidermis, so that it is impossible to ascertain its nature; the web is perfectly smooth.

The *Colour*, so far as preserved, is a dull madder.

Habitat.—Between Prince Edward Island and the Crozets; 1375 fathoms (Station 146). 1 specimen, sex?

Cirroteuthis meangensis, Hoyle.

Cirroteuthis meangensis, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5 vol. xv. p. 234, 1885.

The *Body* is much distorted, but appears to have been subglobular in form. The *mantle-opening* is very small, and fits closely around the base of the *siphon*, which is small and bluntly conical, with a still smaller pimple-like extremity; on either side there extends from the dorso-lateral base a curved fold of membrane, which loses itself in the mantle. The *fins* are about equal in length to the breadth of the body, narrow and pointed at the extremity, and thickened towards the posterior margin. The *dorsal cartilage* is elongated transversely.

The *Head* is exceedingly short, and the *eyes* large and spheroidal.

The *Arms* are subequal, and about three times as large as the body in the present shrunken condition of the specimen; they taper rather rapidly to slender points. The *umbrella* extends on the dorsal side of each arm to within 1 cm. of its extremity, whilst on the ventral side it reaches only four-fifths along it; the arms lie in the umbrella and are not united to it by any intermediate vertical web. The *suckers* are about sixty to seventy in number, small and subequal; they are at equal intervals for the greater part of the arm, but closer near the extremity. The *cirri* are short, stout, and conical, the largest 2 mm. in length; on the ventral arms they commence between the fourth and fifth suckers, and extend to the fiftieth sucker, beyond which there are twenty-one, which gradually decrease; on the dorsal arms the cirri commence between the sixth and seventh suckers, and continue to the fifty-fifth, beyond which there are nine.

The *Surface* is smooth.

The *Colour* of the body is creamy white, of the arms and umbrella deep madder-brown. The suckers and cirri are paler.

Habitat.—Off the Meangis Islands, near the Philippines, 500 fathoms (Station 214). 1 specimen, sex?

Cirroteuthis pacifica, Hoyle.

Cirroteuthis pacifica, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 235, 1885.

The *Body* is almost entirely absent. The *fin* is obovate in form, and thickened along the posterior margin, thin and membranous at the extremity and along the anterior margin. The proximal end of its cartilage is exposed, and presents a long grooved articular surface. One *branchia* is visible, and presents the appearance of a spheroidal nodule with meridional grooves. The *mantle-opening* is circular, and closely embraces the base of the *siphon*, which is long, thin, and conical.

The *Head* is exceedingly short, and the *eye* appears to occupy all the available space between the fin and the arms.

The *Arms* are subequal, thick, rounded, and soft, and taper rapidly towards the extremities. The *umbrella* is attached directly to the arms, which are somewhat more prominent on its inner than on its outer surface; it is attached to the dorsal aspect of every arm almost to the tip, and to the ventral aspect for somewhat more than half its length, and at its attachment is a firm cartilaginous (?) nodule. The *suckers* are about fifty-two in number, and commence close to the oral lip, and the first half-dozen stand near together; halfway up the arms they are farther apart, and the largest are situated opposite the attachment of the membrane to the ventral aspect of the arms; they are prominent, but not so hard and firm as those of *C. magna*. There are faint radial markings upon them. The *cirri* commence on the dorsal arms between the seventh and eighth suckers, and continue till the last; on the ventral arms they commence between the sixth and seventh, and here also are continued to the tips of the arms. They begin as small papillæ, gradually increase in length, attaining the maximum about halfway along the arms.

The *Surface* is smooth.

The *Colour* is a deep purplish madder, paler outside the umbrella and on the fin.

Habitat.—Pacific Ocean, between New Guinea and Australia; surface (Station 181). 1 mutilated specimen, sex ?

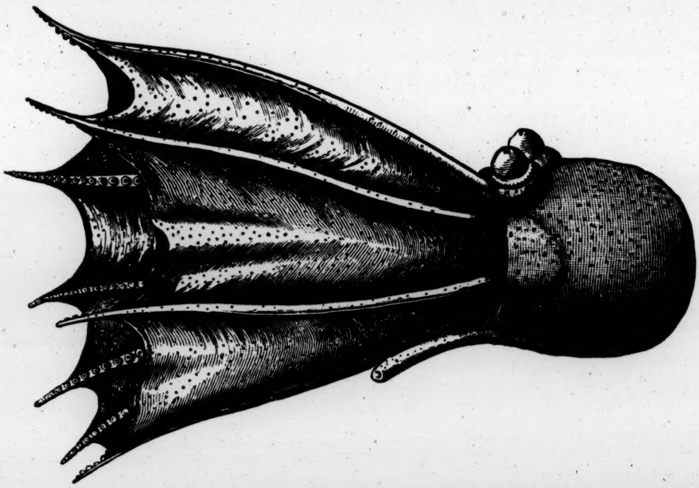
AMPHITRETUS, Hoyle.

Amphitretus pelagicus, Hoyle.

Amphitretus pelagicus, Hoyle, *Ann. and Mag. Nat. Hist.*, ser. 5, vol. xv. p. 235, 1885.

The *Body* is short, rounded, of gelatinous constituency, and semi-transparent. The *mantle* adheres to the sides of the siphon, so that the *mantle-opening*, which is single in all other known Cephalopods, is here divided into two pocket-like openings, which lie one beneath each eye. The *siphon* is very long and narrow, and extends forwards anteriorly to the margin of the mantle, for a distance almost equal to the length of the body, and is a little swollen at the extremity.

The *Head* is not marked off from the body, except by the possession of the *eyes*, which are situated near together on the dorsal surface; they consist of a larger basal spheroid, through the walls of which pigment is clearly visible, upon which stands a smaller very prominent spheroid, white, opaque, and of glistening surface.



Amphitretus pelagicus, Hoyle ($\frac{3}{2}$.)

The *Arms* are equal, and rather more than twice as long as the body; they are slender, and taper at first gradually and then more rapidly to comparatively blunt points. The *umbrella* extends more than two-thirds up the arms, and is thin, delicate, transparent, and much damaged. The *suckers* are firm, muscular cups embedded in the softer tissue of the arms, as in *Cirroteuthis;* there are about twelve placed at some distance apart on that portion of the arm up which the web extends, and eleven closely set, and showing a tendency to biserial arrangement on the free extremities. There are no *cirri*, nor is there any trace of the formation of a *hectocotylus*.

The *Surface* appears to have been quite smooth originally; there is no sign of any cirri or warts.

The *Colour* is a dull yellow, apparently due to preservation in picric acid, and the mantle and umbrella are thickly sprinkled with small brown chromatophores.

Habitat.—Near the Kermadec Islands, South Pacific; on the surface (Station 171). 1 specimen, sex?

2. On Energy in Vortex Motion. By Sir William Thomson.

(*a*) Energy in Vibrations.

(*b*) Unlimited augmentation of Energy of a simply continuous fluid mass in a space of given shape, by changes from and back to this shape.

(*c*) Annulment of Energy under same conditions.

(*d*) Reduction of Energy to absolute minimum in a multiply continuous space of given shape.

(*e*) Unlimited augmentation of Energy in a multiply continuous space.

3. The Theory of Determinants in the Historical Order of its Development. Part I. Determinants in General (1693–1800). By Dr Thomas Muir.

4. On Bipartite Functions. By Dr Thomas Muir.

5. Note on Galois' Theorem regarding the Continued Fraction Representation of the Roots of an Equation. By Dr Thomas Muir.

6. Letter from Professor Michie Smith on the Zodiacal Light, and on the Origin of Atmospheric Electricity.

MADRAS CHRISTIAN COLLEGE'
LABORATORY, 21*st January* 1885.

MY DEAR PROFESSOR TAIT,—I have just got back from an expedition to the top of "Dodabettah" (over 8600 feet), where I have been trying to get the spectrum of the zodiacal light under more favourable circumstances than are possible here. I cannot say that I have met with much success, for though, when the sky was clear, the light was rather brighter than it is here at present, yet I *have* seen it quite as bright here. Still the spectrum was fairly bright, and I don't think that I could have helped seeing bright lines if there had been any to see. I got no photograph. I made, however, one observation, which seems to me to be of importance, and recalls a former observation made in the West Indies. We had sunset about 6 o'clock (M.M.T.), and by 7.15 or 7.30 all trace of the sunset colours (reglow and all) had disappeared, and the Z. L. showed very clear and a little brighter than the milky way. It was broad below, but much narrower above—being about 15° at the horizon and only 6° at an altitude of 30°, and was distinctly visible up to about 60°. Now the point that I noticed was this, that it did not seem to *set* at all, but only to fade out. Even at 10 P.M., four hours after sunset, you could trace it up as high as before, and it had still the same shape—broad below and narrow above. This was not a solitary observation, but was repeated every night on which the moon and the mists—of which I had unfortunately too many—would permit of it. I suppose that if this is really the case, it must belong to the earth, and not to the sun.

I did not confine myself to the Z. L., but did what I could at atmospheric lines and at atmospheric electricity. I made hourly observations of the latter from 6 or 7 A.M. to 10 or 12 P.M., and have got a very fair approximation to the curve of variations for the day between these hours showing a maximum, very well marked, about 2 P.M., and probably a minima near 7 A.M. and 7 P.M.; but the evening observations were so interfered with by mists that they are not of much use for giving points in the curve. But the mist observations seem to me by far the most interesting from another point of view, viz., that of the formation of thunderstorms. Dodabettah, being the highest point of the Niligres, was usually above the clouds in the afternoon, but at times mist was blown over from the comparatively cool easterly side, and was dissipated as it reached the crest of the hill, now in such a dissipating mist, the *air potential was below the normal for that time of day.* On the other hand, with a heavy condensing mist, the *air potential was far above the average.* It seems to me that this is direct evidence in favour of the condensation theory of thunderstorms. I have now an immense mass of electrical observations on hand, some of them of great interest, but it is almost impossible to get the time to make certain ones necessary to connect them or to work them up. I hope, however, now that we have got a new Mathematical Professor, that I will get a little more time for such work.

By the way, I have discovered that the "green sun" spectrum can be exactly represented by combining the spectrum of the sun seen through a fairly thick mist—the sun's disk being still visible—and the spectrum of a moist atmosphere, showing the rain band strongly. This would explain all cases in which the sun has been seen green.

There is one point in regard to measuring atmospheric electricity on which I am not quite clear. In using the water-dropping collector, Thomson seems to say that you must have the reservoir inside, so that part may be on one side and part on the other of the neutral line. I have tried comparisons between the lighted match and the water-dropper entirely outside, and they seemed to agree fairly well. I can get no good exposure here if I simply put the water-dropper at a window, and I would like to have it on the roof of the house, but will that do?—I remain, yours very sincerely,

C. MICHIE SMITH.

7. On a New Form of Chromotrope. By John Aitken, Esq.

8. On an Application of the Atmometer. By Professor Tait.

The Atmometer is merely a hollow ball of unglazed clay, to which a glass tube is luted. The whole is filled with boiled water and inverted so that the open end of the tube stands in a dish of mercury. The water evaporates from the outer surface of the clay (at a rate depending partly on the temperature, partly on the dryness of the air) and in consequence the mercury arises in the tube. In recent experiments this rise of mercury has been carried to nearly 25 inches during dry weather. But it can be carried much farther by artificially drying the air round the bulb. The curvature of the capillary surfaces in the pores of the clay, which supports such a column of mercury, must be somewhere about 14,000 (the unit being an inch). These surfaces are therefore, according to the curious result of Sir W. Thomson (*Proc. R. S. E.* 1870, p. 63), specially fitted to absorb moisture. And I found, by inverting over the bulb of the instrument a large beaker lined with moist filter-paper, that the arrangement can be made extremely sensitive. The mercury surface is seen to become flattened the moment the beaker is applied, and a few minutes suffice to give a large descent, provided the section of the tube be small, compared with the surface of the ball.

I propose to employ the instrument in this peculiarly sensitive state for the purpose of estimating the amount of moisture in the air, when there is considerable humidity; but in its old form when the air is very dry. For this purpose the end of the tube of the atmometer is to be connected, by a flexible tube, with a cylindrical glass vessel, both containing mercury. When a determination is to be made in moist air the cylindrical vessel is to be lowered till the difference of levels of the mercury amounts to (say) 25 inches, and the diminution of this difference in a definite time is to be carefully measured, the atmospheric temperature being observed. On the other hand, if the air be dry, the difference of levels is to be made *nil*, or even negative, at starting, in order to promote evaporation. From these data, along with the constant of the instrument (which must be determined for each clay ball by special experiments), the amount of vapour in the air is readily calculated. Other modes of observation with this instrument readily suggest themselves, and trials, such as it is proposed to make at the Ben Nevis Observatory during summer, can alone decide which should be preferred.

Monday, 2*d March* 1885.

ROBERT GRAY, Esq., Vice-President, in the Chair.

At the request of the Council, an Address on the Recent Progress of the Geological Survey of the United Kingdom was given by Professor Archibald Geikie, F.R.S., Director of the Survey.

On the motion of the Chairman, a vote of thanks was accorded to Professor Geikie for his Address.

PRIVATE BUSINESS.

The following Candidates were balloted for and declared duly elected Fellows of the Society:—Professor Elgar, Dr Orme Masson, and Dr J. M. Macfarlane.

Monday, 16th March 1885.

THOMAS STEVENSON, Esq., Memb. Inst. C.E., President, in the Chair.

The following Communications were read:—

1. Hooke's Anticipation of the Kinetic Theory and of Synchronism. By Professor Tait.

(*Abstract.*)

While collecting materials for a Text-book of the *Properties of Matter*, the author had occasion to consult the very curious pamphlet by Robert Hooke, entitled *Lectures de Potentia Restitutiva, or of Spring* (London, 1678). In this work there is a clear statement of the principle of Synchronism, which was applied by Stokes to the explanation of the basis of Spectrum Analysis. There is also a very remarkable statement of the elementary principles of the modern Kinetic Theory of Gases, the first mention of which is usually fixed sixty years later, and ascribed to D. Bernoulli in his *Hydrodynamica* (Argentorati, 1738).

2. On the Hexagonal System in Crystallography. By Professor Crum Brown.

The forms of the uniaxial systems may be regarded as derived from forms, or parts of forms or combinations, of the regular system by uniform expansion or contraction in a direction parallel to the axis of the uniaxial system, *i.e.*, normal to a face of the cube for the tetragonal, and normal to a face of the octahedron for the hexagonal system. Faces, therefore, which are, in the regular form or combination, at right angles to or parallel to such axis, retain their relative angular position unchanged in the uniaxial form or combination, and can be represented by means of indices referring to the rectangular axes of the regular system, whatever be the amount of the deformation (expansion or contraction). These faces are prism faces, parallel to the axis, and basal faces at right angles to it. All other faces have their angular position affected by the deformation.

These other faces are pyramid faces. Each pyramid face lies between, and in the same zone with, a prism face and a basal face. It may, therefore, be represented by the symbol

$$as + \frac{1}{\rho} bt,$$

where s and t are the symbols of the prism face and the basal face respectively, a and b are small whole numbers, and ρ is the ratio of the length of a line parallel to the axis after, to the length of the line before deformation. We may put

$$\frac{b}{a} = n,$$

when this becomes, for the tetragonal system

$$(hk0) + \frac{1}{\rho} n (001),$$

which is

$$\left(hk \ \frac{n}{\rho}\right)$$

the Miller symbol for a pyramid face in this system, with the ratio of the parameter of z to that of x or y, expressed by ρ. In the hexagonal system the symbol

$$s + \frac{1}{\rho} nt$$

takes the form

$$(hkl) + \frac{1}{\rho} n (111), \text{ where } h + k + l = 0.$$

We may leave ρ understood, as it is constant for the same substance and same temperature, and write this in the contracted form (hkl, n). This gives

$$h + \frac{n}{\rho}, \quad k + \frac{n}{\rho}, \quad l + \frac{n}{\rho},$$

as the coefficients of x, y, and z in the equation of the face referred to the rectangular axes of the regular system. These axes are, of course, not crystallographic axes of the hexagonal system, but some advantages arise from their use. They are rectangular, and therefore the ordinary formulæ of solid geometry can be used; the symbol of the general form (hkl, n), where hk and l are free to change places and change sign together, and n changes sign independently, gives a clear oversight of all the faces of the holohedral form, and enables us to derive from the symbol the various kinds of hemihedry.

3. On the Effect of Temperature on the Compressibility of Water. By Professor Tait.

4. Chemico-Physiological Investigations on the Cephalopod Liver, and its identity as a true Pancreas. By Dr A. B. Griffiths, Ph.D., F.C.S. (Lond. and Paris), Lecturer on Chemistry and Physics, Technical College, Manchester, &c. Communicated by W. E. Hoyle, Esq., M.A.

In a memoir published in the *Chemical News*, vol. xlviii. page 37, and the *Journal of the Chemical Society*, 1884, page 94, I gave some account of a peculiar excretory product found in the *Sepia's* "liver." The product was found to be albumin in pseudo-crystalline aggregations when examined under the microscope. These bodies are not of a constant occurrence in this organ of the *Sepia*. Since the publication of the above paper, which is a year and a half ago, I have made a thorough examination of this organ in *Sepia*, which substaniate and extend the observations of Krukenberg,[*] Fredericq,[†] and Jousset de Bellesme.[‡]

After carefully dissecting it out of the cavity of the body of a fresh Sepia, I performed the following experiments:—

1. A small portion of the organ was placed on starch paste. The starch granules disappeared, with the exception of their celluloid covering, and on treating with water and testing the solution with Fehling's solution, sugar in the dextrose form was found.

2. The organ gave an alkaline reaction to litmus paper.

3. When a small portion of the organ was placed in a tube with a little oil and agitated, an emulsin was produced;—this emulsin had first an alkaline reaction, and after some time became acid, owing to the formation of butyric acid and other acids of the fatty series.

4. The action of it on milk was to render the milk transparent in four hours' time. 15 cubic centimetres of milk were rendered transparent by 6 milligrammes of the tissue of the organ.

5. When a few drops of the secretion of this organ were examined

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[*] *Untersuch. physiol. Inst. Heidelberg,* Bd. i. p. 327, 1878.

[†] *Bull. Acad. Sci. Belgique,* t. lvi. p. 761, 1878; *Rev. Internat. Sci.*, iii. p. 263, 1879.

[‡] *Comptes Rendus,* t. lxxxviii. pp. 304, 428, 1879.

with chemical reagents under the microscope, the following reactions were observed :—

On running in between the slide and cover-slip a solution of iodine in potassium iodide, a brown deposit was the result; and on running in concentrated nitric acid on another slide, containing a drop or two of the secretion, a yellow coloration was formed, due to the formation of xanthoproteic acid. These reactions show the presence of albumin in the secreted fluid of the organ.

Isolation of the Ferment of the Organ.

The process I followed to obtain the ferment or ferments in a crude state from the fluid of the organ was that devised by Wittich and used by Kistiakowsky (Pflüger's *Archiv für Physiologie*, vol. ix. pages 438–459) in his researches on pancreatic ferments. The process consists in hardening the organ in alcohol for three days, and then cutting it up into very small pieces, extracting with *glycerin* and filtering.

On the addition of alcohol to the filtrate, the ferment is precipitated.

The action of this ferment on starch was its complete conversion into dextrose right-handed glucose, which was proved by Fehling's solution; and the formation of crystals ($C_6H_{12}O_6NaClH_2O$) with a solution of sodium chloride, a distinction between levulose or left-handed glucose, which does not form these crystals with salt solution.

The action of the ferment on fibrin from the muscular fibres of a young mouse was the formation of leucin and tyrosin; for on treating the fermented mass with hot water and filtering, a solution is obtained which contains leucin and tyrosin. To this solution was added acetic acid, when acicular crystals are deposited insoluble in ether, but soluble in boiling water, and gave with a neutral solution of mercuric nitrate a red flocculent precipitate, which is characteristic of tyrosin ($C_9H_{11}NO_3$).

The acetic acid solution after precipitating the tyrosin is evaporated, when leucin ($C_6H_{13}NO_2$) is deposited in white shining plates, which melt at 98° C. These shining plates of leucin, when heated with barium oxide, yield amylamine and carbonic dioxide,

$$C_6H_{13}NO_2 = N(C_5H_{11})H_2 + CO_2 .$$

I obtained by digesting the organ itself with boiling water a filtrate containing leucin and tyrosin. The ferment has no action on cellulose. From these investigations I am led to the conclusion that the so-called "liver" of *Sepia officinalis* (the cuttle-fish) is not a true liver,—but is a *pancreas*,—for the juices of the organ in question are purely digestive in function, digesting starch, oil, and similiar bodies; and transforming fibrin into leucin and tyrosin. Then again albumin is found in its secretion, which is characteristic of the pancreatic fluid of the higher animals,—no albumin being found in the liver, for albuminoids are decomposed by that organ. This organ of the Sepia digests oil-globules, and the secretions from a true liver are incapable of digesting fats without the pancreatic fluid.

No glycocholic and taurochloic acids nor glycogen were obtained from the organ, nor could the slightest trace of them be detected in the organ or its secretion. The above investigations prove that this so-called "liver" of the Cephalopod is a true pancreas or digestive organ.

I may mention here that the leucin and tyrosin from the organ I have mounted as microscopic slides.

Monday, 6th April 1885.

JOHN MURRAY, Esq., Ph.D., Vice-President,
in the Chair.

The following Communications were read:—

1. Chromomictors. By Mr John Aitken.

The instruments which we have called chromomictors, have been designed for the purpose of mixing lights of different colours for experiments in physiological optics. There are already a considerable number of instruments for this purpose, but none of them are perfectly satisfactory. We shall here describe one or two new forms of apparatus which have been recently experimented with, and though they may not be suitable for many colour experiments, yet they seem to possess some advantages over several of the instruments now in use. The colours produced by them are very brilliant and beautiful, and the secondary ones, such as yellow, blue, purple, are brilliant and at once recognised by the eye, not being of that dull low tone given by the rotation of coloured discs, while the white is so pure and bright that it cannot be distinguished from that produced under ordinary conditions.

In two forms of this instrument a lens or lenses are used for combining the colours. One of these instruments is simply a camera obscura with a single lens. Suppose now there is a bright white object in front of the lens. The rays coming from any point of that object pass through all parts of the lens, some through the upper half and some through the lower, after which they are again brought to a focus on the field of the camera. If, now, we place in front of the upper half of the lens a transparent red screen, then half of the entering rays will be red and the other half uncoloured, and the image will be of a reddish hue. Let us now place in front of the lower half of the lens another screen of a different colour—say green; the one-half of the entering rays will now be red, and the other half green, and these being combined on the field we get the resultant colour, namely yellow. In this way we can combine on the field of the camera any colours we desire, and by altering the amount of the lens covered by the different screens we can combine the colours in any desired proportion.

Another method of combining colours by means of lenses is to look at the colours through a short focused lens, the coloured objects being placed at a considerable distance. Suppose we have a long camera obscura, closed at one end, and having a short focused lens fitted into the other end. If we now make a small opening anywhere near the axis of the lens in the far end of the camera, so as to admit a little light, and look at the opening through the lens, we shall see a round disc of light, but the disc is not seen exactly in the direction of the opening, but in the axis or centre of the lens. Now make another small opening also near the axis of the lens, say on the side opposite the first. If we now look through the lens, we shall not see two discs of light, but apparently only the one we saw at first, but now it is brighter. We may go on in this way making a number of holes, and the light passing through them will be combined into one round disc of light. Now place over these holes coloured transparent media of any kind, or direct them towards coloured surfaces, then on looking through the lens all the colours will be combined and we shall see the resultant colour.

The following is a short description of an instrument, which I have constructed on this principle, and used with satisfactory results. It consists of a long wooden box about 70 cm. long by 12 cm. deep and 12 cm. wide. In one end of the box are two round openings, placed a short distance apart, one over the other. The lower opening has a diameter of about 5 cm., and has three radial bars fixed in it, dividing the circle into three equal sectors. Into these divisions are fitted pieces of glass of different colours. What these three colours are I shall presently state. The upper opening is of a less diameter than the lower, and is covered with clear glass; the purpose of this opening will be referred to presently.

Each of the sectors in the lower opening is provided with a shutter to regulate the amount of light passing through it. The upper opening is also provided with a movable shutter. Into the opposite end of the box are fitted two eye-pieces, so that, if desired, observations can be made with both eyes at the same time. The lenses of the eye-pieces are similar to a Ramsden eye-piece, and each consists of two plano-convex lenses, mounted at one end of a tube about 30 cm. long. These tubes slide in sockets fixed in the end of the box. The shutters for regulating the quantity of light entering the apparatus are each mounted on the end of a horizontal shaft, which is pivoted at the front end of the box, and passes back to the end next the eye-pieces, where levers are fixed to the end of each, so that each shutter can be moved separately. By this means the amount of the different colours may be conveniently regulated by the observer, while his eye is applied to the instrument. For some observations an opening of 5 cm. for admitting the different coloured lights is too large, as it separates the colours too far; *stops* of different sizes are therefore provided for reducing this opening to the size necessary to suit the experiment.

The colours produced by this instrument are brilliant and beautiful, reminding one of the varied and delicate colours produced by selenite plates and polarised light, and by it we can easily combine the colours so as to give white.

In making white there is, however, always a difficulty in saying when it is white, as there seems to be a range of tints, all of which seem nearly white. To overcome this uncertainty, the instrument is so arranged that either of two methods may be employed to supply the eye with a standard of white light of the correct brilliancy, with which to compare the compound white. By one arrangement the whole field can be alternately filled with white light, or with the white produced by the coloured lights. By this means a very true white can be produced. The adjustment can be made so that no difference can be detected whether the illumination is the result of the coloured lights, or of the white light. Another plan is to fill only one-half the field with the coloured lights, and the other half with white light.

For making these comparative tests, the opening over the coloured glasses is used, and a horizontal division is placed in the box, dividing it into two from end to end. This dividing screen is hinged at the front end, just above the opening filled with the colours. The other end of the screen next the eye-pieces is moved up and down by means of a lever placed outside the box; when the end of the screen next the eye-pieces is raised, the coloured lights fall on the eye-piece, and when it is lowered the coloured lights are cut off, and white light from the upper opening enters the lens. The amount of white light entering this opening requires to be carefully regulated, by means of the shutter, to the same brightness as the white produced by the colours before a match can be made. When properly adjusted it is very difficult to tell whether we are looking at natural white, or the white of the combined colours.

When we wish to illuminate only one-half of the field with white light, the horizontal screen is lowered till it is half-way across the lens, and the division must be carried up to the *stop* in front of the lens. When this is done, the upper half of the field is illuminated with white light, and the lower with the combined colours, and if the eye is kept steady it is simply a question of very nice adjustment to make a perfect match between the two halves, and the field made to look like a white disc with a black bar across the middle. When half the field is filled with white light the conditions are very favourable for observing the various combined colours on the other half; the comparison seems to make them more distinct. This is particularly the case with delicate tints, as they are not easily distinguished if not contrasted with white.

Other forms of apparatus with lenses may be used for combining colours; an ordinary telescope answers fairly well, but an opera-glass will not do. The colours to be combined are held close to the front of the object-glass end of the tube, if they are transparent, such as glass plates; if the colours are opaque, they are placed in a good light, and the tube directed towards them. On looking through the eye-piece we get the result of combining the colours in the proportion in which they are exposed to the end of the tube. The object-glass of the telescope is not necessary for these experiments. The lenses of the eye can also be made to combine the colours without any assistance, but the results are not very satisfactory. If we fix the coloured transparent media with their edges close to each other on a piece of glass, and hold them very near to the eye, confining the entering light to a small angle, we shall then see the colours change through the different combinations, as they are moved across the eye.

With regard to the selection of the colours for these experiments, a few words are necessary. We may of course select any colours we please, and the instruments will combine them for us, and show us the resultant colour or sensation. We shall find that there is an infinite variety of colours which, when combined in proper proportions, will give us white. If, however, we wish to arrange the apparatus, so that it will not only give us white, but will also produce all possible colours, with their shades and tints, then the only three colours which, so far as I am aware, will produce these results, are red, green, and violet. With these three colours we can make white and all colours, such as yellows, blues, and purples of their different shades and tints, but by no combination of colours with which I am acquainted can we make red, green, or violet.

In selecting the colours for the instruments, I took for red a ruby-red glass, dark enough to give only a band in the red when examined with the spectroscope; for green, a glass which passed a narrow band of light in the green of the spectrum, and is about what is known as emerald green; and for violet a glass which passed only the light of the violet end of the spectrum. It is impossible to describe these colours more definitely by means of the lines in the spectrum, as the bands have not a definite boundary. It will be observed that neither the yellow nor the blue light of the spectrum passes into the box, neither of these colours being passed by any of the glasses.

There is considerable confusion as to the names of the colours at the violet end of the spectrum, which for clearness I must refer to here. The colour of the violet glass in the apparatus is often called dark blue, while the colour that is generally known as violet has a reddish hue in it, and when examined with the spectroscope is found to have light of both ends of the spectrum. This reddish-violet is therefore not the violet of the spectrum, but is a compound colour. It is desirable that we should not give the same name to two different colours, and, as the spectrum is an excellent standard of colour, we had better call by the name of violet that colour which corresponds to the violet of the spectrum. It is no objection to this alteration to say that this colour is often called dark blue, because, though it is sometimes called blue, yet it is not the colour which is most generally recognised as blue. All blues contain green, and can be produced by mixing the violet of the spectrum with the green. The blues have a wide range, and run into violet on the one side and into green on the other. Prussian blue, for instance, contains as much green as violet; while ultramarine blue contains comparatively little green. It is, therefore, desirable that the colour, which has neither green nor red in it, and which contains only the light of the violet end of the spectrum, should be called violet.

In experimenting with the apparatus above described, some practice is necessary to enable the observer to keep the eye steady in the correct place, as any movement tends to cause changes in the colour. To overcome this difficulty another form of apparatus has been constructed without lenses. It consists of a rather large camera obscura, divided vertically into two compartments by a thin metal partition. One compartment is lighted by an opening in the top, provided with a shutter to regulate the brilliancy of the light falling on a piece of white paper placed on the bottom of this compartment. The other compartment is lighted by a large circular window, also in the top. This window is divided into three sectors, and is glazed with red, green, and violet glasses. Each sector is provided with a shutter to regulate the amount of light of each colour entering and falling on the white paper at the bottom of this compartment. An opening is made in one side of the box, and arrangements made, so that while light is excluded, the observer can look down on the bottom of both compartments of the box, and observe the colour of the illumination on the paper. When one eye is placed directly in front of the thin metal partition it can see the bottom of both compartments at once, divided by the thin line of the partition. The paper in the one compartment, lighted by white light, is thus only separated by a fine line, from the other one illuminated by the coloured lights, and comparison is thus easily made. When working with this colour box it is taken outside or placed at an angle of about 45 degrees at an open window. By opening the shutters over the coloured glasses to the proper degree, the paper on the bottom of this compartment may be made to appear of any colour or tint we may desire. The other compartment of the box is illuminated with white light, and the shutter is opened to the desired amount to provide the eye with a standard of white.

When the three colours are combined in the proper proportions, so perfect a white can be produced with this apparatus that it is impossible to distinguish between the standard white and the white produced by the three brilliant colours. In order to get this perfect match of the two whites, great care must be taken in the construction of the apparatus. The surface of the interior of the box must be *dead* white; if there is the smallest amount of polished surface reflection, such as that given by most paints, then the field of observation will not be flat, but will be differently tinted at different points. If the surfaces in the interior were perfectly flat, these reflections would not interfere with the result; but, owing to the unavoidable curvatures of these surfaces, the colours are unequally reflected to the paper. The source of the illumination required for working this box ought to have as wide an angle as possible. A bright sky does very well, but if the sun shines on the instrument, ground glass or thin white paper must be hung up in front of the glasses.

One advantage of this colour box is the ease to the eye with which the observations can be made, and the secondary colours, such as yellow, blue, and purple, produced by it are brilliant. If we open the red and green shutters, the colour produced is a canary-yellow of considerable brightness and strength. To get this bright yellow we must be very careful in the selection of the glasses in the window, and also be very certain that every ray from the violet glass is excluded by its shutter, as a very little violet light causes the yellow to grow pale and change to white.

An interesting and beautiful experiment can be made with this box. If, after having adjusted the colours to produce white, we introduce an opaque body into the compartment lighted with the coloured glasses, there is at once formed a striking display of prismatic-like colours. The paper on the bottom of the box is no longer white, but covered with most varied and brilliant hues, caused by the penumbra of the opaque body being lighted with different colours on the different sides. These effects can be intensified by placing over the coloured glasses a shutter with three round openings cut in it, so as to separate the sources of the different coloured lights. The colours on the bottom of the box will now be found to be further separated, more distinct, and fuller in tone. The form of the shadow-producing body may be varied, but perhaps the most beautiful effects are got when it is in the shape of a flat ring, placed a short distance from the paper. With that form it is interesting to follow the manner in which the different colours are produced. Confining our attention to the light passing through the centre of the ring, we can distinguish on the bottom of the box three overlapping circles of light—one red, another green, and the third violet; where the red and green overlap a brilliant yellow is produced; where the green and the violet overlap are the blues; and the red and the violet give the purples; while in the centre, where all three circles overlap, white is produced. The purity of these colour effects is increased by having the inside of the box painted of a dull black, as this destroys all internal reflections which would dilute and weaken the colour effects. In order that the same apparatus may do for both kinds of experiments, the inside of the box is provided with two folding screens, each the same size as the side of the compartment; one side of each screen is black and the other white. By folding the screens one way, the inside of the box is made all white, for experiments in combining colours; and by folding them the other way, the interior is made all black, to prevent reflections which would destroy the brilliancy of the coloured shadow experiments.

These colour experiments explain why there is so little appearance of coloured light in our Cathedrals, even when there is much coloured light entering through the stained glass windows, and they show us that we might glaze the windows with the most brilliantly coloured glass, and yet the light in the interior might be quite white.

It is hoped these colour-combining instruments will be found useful for teaching physiological optics, for experiments on colour perception, and also for measuring the qualities of lights from different sources. The possibility of making a perfect match, and the extreme nicety of the adjustment necessary to produce a perfect balance, indicate that these instruments are capable of considerable accuracy.

2. On Chlorophyll from the Deep Sea. By W. W. Hartley, F.R.S., Royal College of Science, Dublin.

The shells of certain molluscs which are obtainable on the seashore between tides are seen to be more or less coloured green. I have noticed a fine rich colour on the operculum of a *Turbo* from the Pacific Ocean, which had been killed and the shell preserved dry. The shell has a high polish, the colour is the rich green of an ivy leaf, and the pigment is contained in the shell material. When the mollusc dies, and the shell is left exposed to air, light, and water, the green becomes a brownish-yellow colour. Some specimens of spirit, which had been poured off shells and fragments of coral obtained by dredging in deep waters, were examined by me for Mr J. Y. Buchanan. He informed me that some of the shells were of a beautiful green colour, but there was no appearance of any growth of algæ adhering to them. When placed in spirit, the colour became dissolved. After evaporation of the spirit, a mixture of green and yellow pigments was found in the residue, and nothing else. The green pigment was insoluble, while the yellow appeared to be slightly soluble in water. On shaking the substance with hydrochloric acid and ether, I did not at first get Fremy's reaction; but subsequently shaking ether with the strongest hydrochloric acid and then adding an ethereal solution of the colouring matter, a bluish aqueous solution was formed, and a brownish-yellow solution in ether. The general reactions indeed were those of blue chlorophyll, which has been described by Sorby as existing in the leaves of land plants and in certain sea-weeds. The yellow substance was one of the colouring matters which result from the alteration of chlorophyll. On heating the alcoholic solution with zinc dust in a sealed tube at 100° C., the green colour became very much brighter, and the yellow being removed the tint was much purer. After four hours the liquid was partially decolorised. The spectrum of the solution was carefully examined, the details of the examination here follow.

Spectrum of a Green Colouring Matter derived from Ocean Shells.—The shells were preserved in alcohol, which dissolved the colouring matter. A bottle was received from Mr J. Y. Buchanan in January 1884, which bore the following label:—

"S.S. 'Dacia,' 17th Nov. 1883.
Lat. 33°·42′ N. Long. 14°·7′ W.
Depth, 533 fathoms. Coral Patch."

The original liquid, which measured about three ounces, was examined just as it was poured from the bottle, and again after undergoing concentration by evaporation. It was of a pale green colour and slightly yellowish, but after being concentrated it became a dull olive-green.

The spectrum was observed with a small spectroscope of the usual form, fitted with a compound prism capable of dividing the D lines; the measurements were made upon a divided arc; a pointer shaped like a broad finger was placed in the eye-piece. One side of the finger was perfectly straight and vertical, the other was curved and brought to a fine point like a pen nib; when this was moved over an absorption band, a reading was taken as soon as the fine point ceased to be visible. The arc measurements were reduced to wave-lengths and oscillation frequencies by means of two interpolation curves obtained from those flame and spark spectra recommended as affording good lines of reference in the Report on Spectrum Analysis, in the volume published by the British Association for the year 1881:—

Thickness of layer of liquid.	Description of Spectrum.	Arc measurements.	Oscillation frequencies.	Wave-lengths.
10 mm.	Termination in the red, .	41°·4′	1411	7085
	ABSORPTION BAND in the red overlying the Solar line C.			
	Less refrangible edge, .	41°·20′	1490	6711
	More refrangible edge, .	41°·35′	1560	6410
	Visible spectrum ends at .	43°·48′	2038	4907

A solution of chlorophyll, prepared from dried *Anacharis* by extraction with pure dried ether, was of a very bright green colour, when approximately of the same shade, and when seen in layers of 10 mm. thickness, the spectrum measurements corresponding to the above were the following:—

Thickness of layer of liquid.	Description of Spectrum.	Arc measurements.	Oscillation frequencies.	Wave-lengths.
10 mm.	Termination in the red, .	41°·4′	1411	7085
	ABSORPTION BAND.			
	Less refrangible edge, . .	41°·18′	1480	6754
	More refrangible edge, . .	41°·35′	1560	6410
	Visible spectrum ends at .	44°·14′	2100	4700

In February 1884 I received from Mr John Murray a series of twelve bottles of spirit in which some of the "Challenger" dredgings had been preserved for periods of eight to ten years. They were labelled as follows:—

1. St. 186, 8th Sept. 1874; Cape York; 8 fathoms.
2. St. 151, 7th Feb. 1874; off Heard Isle; 75 fathoms.
3. 7th March 1875; Admiralty Isle; 16–25 fathoms.
4. 22d July 1874; Tongatabou; 18 fathoms.
5. St. 209, 22/1/75; Zebu; 95–100 fathoms.
6. St. 157, 3d March 1874; 1950 fathoms.
7. 23d Sept. 1874; off Arrou Isle; 800 fathoms.
8. 16th October 1874; 17 fathoms.

9. St. 344, 3d April 1876; 420 fathoms.
10. 25th April 1876; off St Vincent; 15–20 fathoms.
11. January 1874; Royal Sd. Kerguelen; 30 fathoms.
12. H.M.S. "Triton" St. 10, 24/8/82; 516 fathoms.

As none of the liquids were strongly coloured, they were examined in a tube eight inches in length, closed at the ends with parallel plates of glass. Though all the specimens had the yellowish-brown or brownish-green appearance of altered chlorophyll, only three showed measurable bands, namely, those from 15–20 fathoms, 516 fathoms, and from 800 fathoms.

No. of Specimen.	Description of Spectrum.	Arc measurements.	Oscillation frequencies.	Wave-lengths.
10	Termination in the red	41°·2′	1400	7143
	ABSORPTION BAND in the red (1), . .	41°·18′ to 41°·27′	1481 to 1524	6752 to 6561
	The same with rather less light (2), . .	41°·20′ to 41°·27′	1490 to 1524	6711 to 6561
	Very faint band in the yellow, discernible about . . .	41°·53′ to 42°·3′	1635 to 1676	6116 to 5966
	Very faint band in the green, . . .	42°·45′ to 43°·8′	1833 to 1914	5455 to 5224
	Spectrum darkens at this point, . .	43°·30′	1980	5050
	Spectrum terminates at	44°19′	2114	4730
12	ABSORPTION BAND in the red, . . .	41°·18′ to 41°·30′	1481 to 1536	6752 to 6510
	No further accurate measurements could be taken.			
7	ABSORPTION BAND in the red, . . .	41°·18′ to 41°·28′	1481 to 1530	6752 to 6536
	No further accurate measurements could be taken.			

The source of illumination was a bat's-wing gas-burner placed end on to the slit.

No. 10, 25th April 1876, off St Vincent, 15–20 fathoms. Colour of liquid as seen in tube eight inches long, brownish olive-green.

No. 12. H.M.S. "Triton" 516 fathoms. St. 10. 24/8/82. Colour of liquid as seen in tube, brownish shade of orange colour.

No. 7. Off Arrou Isle, 23d Sept. 1874, 800 fathoms. Colour, brownish-orange.

The close agreement of the measurements of the band in the red with those of the same band in fresh chlorophyll is evidence of the nature of the substance. It may be of interest to point out that the green colouring matter from liquid No. 10 was insoluble in water, but soluble in alcohol and ether; boiling with dilute sulphuric acid showed that it was not a glucoside, as it had subsequently no action on Fehling's solution. The yellow colouring matter from the same specimen was soluble in water; it had a very feeble action on Fehling's solution. Both specimens No. 12 and No. 7 yielded yellow residues soluble in water, which reduced copper solution at once on boiling.

According to Sorby, blue chlorophyll in strong solutions shows three bands at the red end of the spectrum, the least refrangible being the most intense (*Proc. Roy. Soc.*, vol. xxi. p. 442). I have observed only one band in solutions corresponding in strength to the liquids numbered 12, 10, and 7, when examined in the same way, and it is that which is figured in the paper just quoted; the bands in the yellow and green are due to the colouring matters derived from chlorophyll.

An examination was made of living green sea-weed brought out of the Irish Channel by trawlers. The fronds being thin and filmy, can be easily laid upon glass and placed before the slit of the spectroscope to be examined in layers of different thicknesses by direct sunlight. One thickness showed a dark band in the red, with an extension towards the orange, which was undoubtedly the second and weaker band of blue chlorophyll; but there were no traces of bands in the orange and green. The spectrum ended just beyond the magnesium green triplet *b*. Two thicknesses caused the band to be much increased in density; no band in the orange; green practically unaltered, the end of the spectrum nearly approaches the triplet in the green. Three and four thicknesses transmitted no trace of red, but only the green and part of the yellow rays. No band in the green was observed at all.

When the sea-weed was rapidly dried and extracted with pure dry ether, no other bands were seen with distinctness; but an alcoholic solution of the colouring matter from the same specimen, and much stronger, exhibited three absorption bands.

Pure dry ether does not take up so much colouring matter as alcohol, and it appears as if the ethereal solution contained but two substances, namely, blue chlorophyll, and a yellow body which causes absorption only beyond the *b* group. The spectrum of the ethereal solution very closely resembles that of the fronds of the living sea-weed, while the alcoholic solution differs therefrom.

The Absorption Spectrum of Chlorophyll as seen in the Fronds of living Sea-Weed.

Description of Spectrum.	Arc measurements.	Oscillation frequencies.	Wave-lengths.
Termination in the red, .	41°·	1390	7194
ABSORPTION BAND in the red, ...	41°·15′ to 41°·35′	1466 to 1560	6820 to 6410
Termination in the blue,	43°·39′	2010	4975
A second series of Measurements.			
ABSORPTION BAND in the red, ...	41°·16′ to 41°·37′	1468 to 1568	6812 to 6377

The Absorption Spectrum of Chlorophyll extracted from the dried Sea-Weed by Alcohol.

Description of Spectrum.	Arc measurements.	Oscillation frequencies.	Wave-lengths.
Termination of spectrum in red, . . .	41°·	1390	7194
ABSORPTION BAND in red,	41°·17′ to 41°·39′	1475 to 1576	6779 to 6343
Feeble *Absorption band* in the orange, . .	41°·52′ to 42°·	1631 to 1664	6131 to 6008
Feeble *Absorption band* in the green, . .	42°·23′ to 42°·57′	1753 to 1875	5702 to 5333

The bands in the orange and green were much weaker in the green colouring matter extracted from sea-weed than they usually appear when an alcoholic extract of leaves is examined, as they are caused in the latter case by substances of the xanthophyll group (Sorby, *loc. cit.*) and alteration products.

I am indebted to Mr John Murray for the following note, in answer to a query as to the possibility of floating molluscs containing colouring matters of the nature of chlorophyll, and after dying falling to the bottom of the ocean :—

"The green colouring matter from the 'Dacia,' 533 fathoms, must have come, I think, from the corals and other organisms lying on the bottom, and not from the floating molluscs or other surface organisms."

The occurrence of chlorophyll in the depths of the ocean does not necessarily imply the presence of plant life, since spongilla have been found coloured by this substance (Ray Lankester, *Quarterly Journal of Microscopical Science*, 1882, p. 253), but it opens up the question whether rays of light are at all necessary for its production.

The penetration of light to the bottom of the deep sea is a possible if not a probable phenomenon. It it true that in the clear waters of Lake Leman photographic dry-plates are affected at mid-day at no depth beyond 120 fathoms; but Messrs Fol and Sarasin are led to suppose that the greater transparency of sea-water will admit of the extreme limit of luminous rays reaching a still lower level. ("Sur la Pénétration de la Lumière du Jour dans les Eaux du Lac de Genève," *Comptes Rendus*, xcix. p. 783.)

Facts at present at our disposal do not warrant the assumption that light cannot pass beyond this depth, for though the violet and ultra-violet certainly do not, as proved by Messrs Fol & Sarasin, yet water may transmit the green rays under circumstances which enable it to obscure the rest of the spectrum—the less refrangible portion by absorption, the more refrangible partly by absorption and partly by scattering. (See M. J. L Soret's "Memoir Sur la Couleur de l'Eau," *Journal de Physique*, 2nd series, vol. iii. 1884.)

The reason why we have very little evidence at present of the penetration of great depths by green rays is owing to the fact that gelatine emulsion containing silver bromide is very slightly sensitive to such light. If any rays are necessary for the production of chlorophyll in plant life, which is extremely doubtful, they are those in the yellow and green lying between the solar lines D and E, this portion of the spectrum being transmitted by the substance. We have accordingly no grounds for the conclusion that because chlorophyll has been found at a depth of 553 fathoms, even if the pigment be produced *in situ*, therefore light has not penetrated so far, and has not operated in its formation.

3. On the Termite as the Tropical Analogue of the Earth-Worm. By Professor Henry Drummond.

My object in this paper is to call attention to a few observations which occurred to me during a recent scientific survey of part of Central Africa, regarding the larger economy of tropical nature. And I wish especially, and with much deference, to attempt to supplement a well-known theory of Mr Darwin's, by bringing forward another claimant to the honour of being, along with the earth-worm, the leading organic agricultural and geological agent in nature.

By means of inorganic agencies—the air, the frost, and the rain—nature has no difficulty in any part of the world in preparing surface films of disintegrated soil for the growth of vegetable life. But though the elaboration of a surface film by these agencies would produce crops for a few years, a much more radical system of agriculture must be in operation before a prolonged succession of crops can be kept up from year to year, and from century to century. The lower layers of soil, exhausted with bringing forth, must be constantly transferred to the top; while the upper layers, restored, disintegrated, and saturated with the fertilising products of organic decomposition, must be lowered down to where the rootlets spread themselves in the under soil. The patient series of observations by which Mr Darwin established the conclusion that this function was discharged by the earth-worm are too well-known for repetition. On every acre of land in England, Mr Darwin calculates that more than 10 tons of dry earth are passed through the bodies of worms, and brought to the surface every year; and he assures us that the whole soil of the county must pass and repass through their bodies every few years. "The plough," he says, "is one of the most ancient and most valuable of man's inventions; but long before he existed the land was, in fact, regularly ploughed by earth-worms. It may be doubted whether there are many other animals which have played as important a part in the history of the world as have these lowly organised creatures."*

Now, while admitting to the fullest extent the influence of worms in countries which enjoy a temperate and humid climate, it can scarcely be allowed that the same influence is exerted, or can possibly be exerted, in tropical lands. No man was less in danger of taking a provincial view of nature than Mr Darwin, and in discussing the earth-worm he has certainly collected evidence from different parts of the globe. He refers, although sparingly, and with less than his usual wealth of authorities, to worms being found in Iceland, in Madagascar, in the United States, Brazil, New South Wales, India, and Ceylon. But his facts, with regard especially to the influence on the large scale of the worm in warm countries, are few or wholly wanting. Africa, for instance, the most tropical country in the world, is not referred to at all; and where the activities of worms in the tropics are described, the force of the fact is modified by the statement that these are only exerted during the limited number of weeks of the rainy season.

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* *Vegetable Mould and Earth-Worms*, p. 313.

The fact is, for the greater portion of the year in the tropics the worm cannot operate at all. The soil, baked into a brick by the burning sun, absolutely refuses a passage to this soft and delicate animal. During the brief period of the rainy season worms undoubtedly carry on their function in some of the moister tropical districts; and in the sub-tropical regions of South America and India worms, small and large, appear with the rains in endless numbers. But, on the whole, the tropics proper seem to be poorly supplied with worms. In Central Africa, though I looked for them often, I never saw a single worm. Even when the rainy season set in, the closest search failed to reveal any trace either of them or of their casts. Nevertheless, so wide is the distribution of this animal, that in the moister regions even of the equatorial belt one should certainly expect to find it. But the general fact remains. Whether we consider the comparative poorness of their development, or the limited period during which they can operate, the sustained performance of the agricultural function by worms, over large areas in tropical countries, is impossible.

Now, as this agricultural function can never be dispensed with, it is more than probable that nature will have there commissioned some other animal to undertake the task. And the animal we are in search of, and which I venture to think equal to all the necessities of the case, is the termite, or white ant. This animal, the popular name of which is misleading, seeing that it belongs to the

Termitidæ, and is not an ant at all, is a small ant-shaped insect, with a bloated, yellowish-white abdomen, and a somewhat large thorax, oblong-shaped, and in colour a disagreeable oily brown. There are three or four different species of termites, and their geographical range is probably second in extent only to that of the earth-worm. The white ants are found in enormous numbers throughout the length and breadth of Africa and India; they occur in South America, Australia, and Ceylon; and every sub-tropical region is more or less infested by them. The insects themselves, owing to a peculiarity in their habits, are rarely seen, but throughout large districts in Africa the ground is literally living with them. On the Tanganyika plateau I have camped at night on ground which was as hard as adamant, and as innocent of white ants apparently as the pavement of St Paul's, and wakened next morning to find all the wooden articles in my tent almost gnawed to pieces. The termite lives almost exclusively upon dried wood, and it is upon a peculiarity in its mode of securing this that its agricultural and geological functions largely depend, so far at least as Central Africa is concerned.

I have just said that the white ant is never seen above the surface of the ground; and yet, without coming to the surface, it cannot secure the decaying branches, fallen trunks, and dead wood on which it lives. How does it solve the difficulty? It takes some of the ground out along with it. I have seen white ants working on the top of a high tree, and yet they were underground. They took up some of the ground with them to the tree-top; just as the Esquimaux heap up snow, building it into the low tunnel-huts in which they live, so the white ants collect earth, only in this case not from the surface but from some depth underneath the ground, and plaster it into tunnelled ways. Occasionally these run along the ground, but more often mount in endless ramifications to the top of trees, meandering along every branch and twig, and here and there debouching into large covered chambers which occupy half the girth of the trunk. Millions of trees in some districts are thus fantastically plastered over with tubes, galleries, and chambers of earth, and many pounds weight of subsoil must be brought up for the mining of even a single tree. The building material is conveyed by the insects up a central pipe with which all the galleries communicate, and which at the downward end connects with a series of subterranean passages leading deep into the earth. The method of building the tunnels and covered ways is as follows:—At the foot of a tree the tiniest hole cautiously opens in the ground close to the bark. A small head appears with a grain of earth clasped in its jaws. Against the tree trunk this earth-grain is deposited, and the head is withdrawn. Presently it reappears with another grain of earth; this is laid beside the first, rammed tight against it, and again the builder descends underground for more. The third grain is not placed against the tree but against the former grain; a fourth, a fifth, and a sixth follow, and the plan of the foundation begins to suggest itself as soon as these are in position. The stones or grains, or pellets of earth, are arranged in a semi-circular wall, the termite, now assisted by three or four others, standing in the middle between the sheltering wall and the tree, and working briskly with head and mandible to strengthen the position. The wall, in fact, forms a small moon-rampart, and as it grows higher and higher it soon becomes evident that it is going to grow from a low battlement into a long perpendicular tunnel running up the side of the tree. The workers, safely ensconced inside, are now carrying up the structure with great rapidity, disappearing in turn as soon as they have laid their stone and rushing off to bring up another. Each stone as it is brought to the top is first of all covered with mortar. Of course, without this the whole tunnel would crumble into dust before reaching the height of half an inch; but the termite pours over the stone a moist sticky secretion, turning the grain round and round with its mandibles until the whole is covered with slime. Then it places the stone with great care upon the top of the wall, works it about vigorously for a moment or two till it is well jammed into its place, and then starts off instantly for another load.

Peering over the growing wall one soon discovers one, two, or more termites of a somewhat larger build, considerably longer, and with a very different arrangement of the parts of the head, and especially of the mandibles. These important-loooking individuals saunter about the rampart in the most leisurely way, but yet with a certain air of business, as if perhaps the one was the master of works and the other the architect. But closer observation suggests that they are in nowise superintending operations, nor in any immediate way contributing to the structure, for they take not the slightest notice either of the workers or the works. They are posted there in fact as sentries, and there they stand, or promenade about, at the mouth of every tunnel, to keep at bay the warlike hordes of *Formicidæ*—the real ants—who forage about in every tropical forest in unnumbered legions. To every hundred workers in a white ant colony, which numbers many thousands of individuals, there are perhaps two of these fighting men. The division of labour here is very wonderful, and the fact that besides these two specialised forms there are in every nest two other kinds of the same insect, the kings and queens, shows the remarkable height to which civilisation in these communities has attained.

Now where is this tunnel going to, and what object have the insects in view in ascending this lofty tree? Thirty feet from the ground, across innumerable forks, at the end of a long branch are a few feet of dead wood. How the ants know it is there, how they know its sap has dried up, and that it is now fit for the termites' food, is a mystery. Possibly they do not know, and are only prospecting on the chance. The fact that they sometimes make straight for the decaying limb argues in these instances a kind of definite instinct; but, on the other hand, the fact that in most cases the whole tree, in every branch and limb, is covered with termite tunnels, would show perhaps that they work most commonly on speculation, while the number of abandoned tunnels, ending on a sound branch in a *cul-de-sac*, proves how often they must suffer the usual disappointments of adventurers.

The extent to which these insects carry on their tunnelling is quite incredible until one has seen it in nature with his own eyes. The tunnels are perhaps about the thickness of a small-sized gas-pipe, but there are junctions here and there of large dimensions, and occasionally patches of earth-work are found embracing nearly the whole trunk for some feet. The outside of these tunnels, which are never quite straight, but wander irregularly along stem and branch, resembles in texture a coarse sand-paper; and the colour, although this naturally varies with the soil, is usually a reddish-brown. The quantity of earth and mud plastered over a single tree is often enormous; and when one thinks that it is not only an isolated specimen here and there that is frescoed in this way, but often the whole of the trees of a forest, some idea will be formed of the magnitude of the operations of these insects and the extent of their influence upon the soil which they are thus ceaselessly transporting from underneath the ground.

In travelling through the great forests of the Rocky Mountains or of the Western States, the broken branches and fallen trunks strewing the ground breast-high with all sorts of decaying litter frequently make locomotion impossible. But in an African forest not a fallen branch is seen. One is struck at first at a certain clean look about the great forests of the interior, a novel and unaccountable cleanness, as if the forest-bed was carefully swept and dusted daily by unseen elves. And so, indeed, it is. Scavengers of a hundred kinds remove decaying animal matter—from the carcase of the fallen elephant to the broken wing of a knat—eating it, or carrying it out of sight, and burying it in the deodorising earth. And these countless millions of termites perform a similar function for the vegetable world, making away with all plants and trees, all stems, twigs, and tissues, the moment the finger of decay strikes the signal. Constantly in these woods one comes across what appears to be sticks and branches and bundles of faggots, but when closely examined they are seen to be mere casts in mud. From these hollow tubes, which preserve the original form of the branch down to the minutest knot or fork, the ligneous tissue is often entirely removed, while others are met with in all stages of demolition. In attacking a small branch the insects start apparently from two centres. One company attacks the inner bark, which is the favourite morsel, leaving the coarse outer bark untouched, or more usually replacing it with grains of earth atom by atom as they eat it away. The inner bark is gnawed off likewise as they go along, but the woody tissue beneath is allowed to remain to form a protective sheath for the second company who begin work at the centre. This second contingent eats its way outward and onward, leaving a thin tube of the outer wood to the last as props to the mine till they have finished the main excavation. When a fallen trunk lying upon the ground is the object of attack, the outer cylinder is frequently left quite intact, and it is only when one tries to drag it off to his campfire that he finds he is dealing with a mere hollow tube a few lines in thickness filled up with mud.

But the works above ground represent only a part of the labours of these slow-moving but most industrious of creatures. The arboreal tubes are only the prolongation of a much more elaborate system of subterranean tunnels which extend over large areas and mine the earth sometimes to a depth of many feet or even yards.

The material excavated from these underground galleries and from the succession of doomed chambers—used as nurseries and granaries—to which they lead, has to be thrown out upon the surface. And it is from these materials that the huge ant-hills are reared, which form so distinctive a feature of the African landscape. These heaps and mounds are so conspicuous that they may be seen for miles, and so numerous are they and so useful as cover to the sportman, that without them in certain districts hunting would be impossible. The first things, indeed, to strike the traveller in entering the interior are the mounds of the white ant, now dotting the plain in groups like a small cemetery, now rising into mounds singly or in clusters, each 30 or 40 feet in diameter, and 10 or 15 in height, or again standing out against the sky like obelisks, their bare sides carved and fluted into all sorts of fantastic shapes. In India these ant-heaps seldom attain a height of more than a couple of feet, but in Central Africa they form veritable hills, and contain many tons of earth. The brick houses of the Scotch mission-station on Lake Nyassa have all been built out of a single ants' nest, and the quarry from which the material has been derived forms a pit beside the settlement some dozen feet in depth. A supply of bricks as large again could probably still be taken from this convenient depôt, and the missionaries on Lake Tanganyika and onwards to Victoria Nyanza have been similarly indebted to the labours of the termites. In South Africa the Zulus and Kaffirs pave all their huts with white-ant earth; and during the Boer war our troops in Praetoria, by scooping out the interior from the smaller beehive-shaped ant-heaps, and covering the top with clay, constantly used them as ovens. These ant-heaps may be said to abound over the whole interior of Africa, and there are three or four distinct varieties. The most peculiar, as well as the most ornate, is a small variety from 1 to 2 feet in height, which occurs in myriads along the shores of Lake Tanganyika. It is built in symmetrical tiers, and resembles a pile of small rounded hats, one above another, the rims depending like eaves, and sheltering the body of the hill from rain. To estimate the amount of earth per acre raised from the water-line of the subsoil by white ants would not in some districts be an impossible task, and it would be found probably that the quantity at least equalled that manipulated annually in temperate regions by the earth-worm.

Let me now attempt to show the way in which the work of the termites bears upon the natural agriculture and geology of the tropics. Looking at the question from the large point of view, the general fact to be noted is, that the soil of the tropics is in a state of perpetual motion. Instead of an upper crust, moistened to a paste by the autumn rains, and then baked hard as adamant in the sun; and an under soil, hermetically sealed from the air and light, and inaccessible to all the natural manures derived from the decomposition of organic matters—these two layers being eternally fixed in their relation to one another—we have a slow and continued transference of the layers always taking place. Not only to cover their depredations, but to dispose of the earth excavated from the underground galleries, the termites are constantly transporting the deeper and exhausted soils to the surface. Thus there is, so to speak, a constant circulation of earth in the tropics, a ploughing and harrowing, not furrow by furrow and clod by clod, but pellet by pellet and grain by grain. Some idea of the extent to which the underlying earth of the tropical forests is thus brought to the surface will have been gathered from the facts already described; but no one who has not seen it with his own eyes can appreciate the gigantic magnitude of the process. Occasionally one sees a whole trunk or branch, and sometimes almost an entire tree, so swathed in red mud that the bark is almost completely concealed, the tree looking as if it had been taken out bodily and dipped in some crystallising solution. It is not only one tree here and there that exhibits the work of the white ant, but in many places the whole forest is so coloured with dull red tunnels and patches as to give a distinct tone to the landscape—an effect which, at a little distance, reminds one of the *abend-roth* in a pine forest among the Alps. Some regions are naturally more favourable than others to the operations of the termites, and to those who have only seen them at work in India, or in the lower districts of Africa, this statement may seem an exaggeration. But on one range of forest-clad hills on the great plateau between Lake Nyassa and Tanganyika I have walked for miles through trees, every one of which, without exception, was ramified, more or less, with tunnels. The elevation of this locality was about 5000 feet above the sea, and the distance from the equator some 9°; but nowhere else have I seen a spot where the termites were so completely masters of the situation as here. If it is the case that in these, the most elevated regions of Central Africa, the termite colonies attain their maximum development, the fact is of much interest in connection with the geological and agricultural functions which they seem to serve; for it is here precisely before the rivers have gathered volume, that alluvium is most wanting; it is here that the tiny headwaters of these same rivers collect the earth for subsequent distribution over the distant plains and coasts; and though the white ant may itself have no power, in the first instance, of creating soil, as a denuding and transporting agent its ministry can scarcely be exaggerated.

The direct relation of the termites' work to denudation will still further appear, if we try to imagine the effect upon these accumulations of earth-pellets and grains of an ordinary rainy season. For two or three months in the tropics, though intermittently, the rains lash the forests and soils with a fury such as those in temperate climates have little idea of; and though the earth-works, and especially the larger ant-hills, have marvellous resisting properties, they are not invulnerable, and must ultimately succumb to denuding agents. From a geological point of view, these ant-hills, which occur in such numbers all over Africa that the whole country may be regarded as one vast ant-city, are to be looked upon simply as heaps of decomposed rock waiting to be removed from the surface of the earth. The tunnels, again, being only required for a temporary purpose, are made substantial enough only to last the occasion; and in spite of the natural glue which cements the pellets of earth together, the structure, as a whole, after a little exposure, becomes extremely friable, and crumbles to pieces at a touch. Many trees, from which all the earthy material has been long since swept away, still bear upon their trunk and branches the long, irregular stain which indicates former termite sites. And although the insects will sometimes repair in a single night the ravages of the day, there must be thousands of miles of disused workings annually left to their fate. When the earth-tubes crumble into dust in the summer season, the débris is scattered over the country by the wind, and in this way tends to increase and refresh the soil. During the rains, again, it is washed into the rivulets and borne away to fertilise with new alluvium the distant valleys, or carried downward to the ocean, where, along the coast-line, it "sows the dust of continents to be."

4. On Peroxides of Zinc, Cadmium, Magnesium, and Aluminium. By J. Gibson, Ph.D., and R. M. Morrison, D.Sc.*

(Read July 5, 1880.)

During the course of an investigation on the cerite metals we found that peroxide of hydrogen, added to the precipitated hydrates in presence of excess of alkali, is a very delicate test for cerium, even in presence of Lanthanum and Didymium, owing to the formation of a dark yellow or brown compound, probably a peroxide, which differs from that produced by chlorine under the similar circumstances.

While examining a residue obtained by boiling a solution of La and Di with magnesite, to free it from cerium, we observed, on using this test, the following reaction, which led us to suspect the existence of a peroxide of magnesium, namely, that when H_2O_2 was added to a solution containing chloride of magnesium and free ammonia, a copious white precipitate was formed. On repeating the experiment with solutions of a number of metals, we obtained precipitates which possessed the properties of peroxides.

On referring to the literature of the subject, we found that the greater number had been already prepared in a similar manner by Thenard and others; but we were unable to find any mention of peroxides of cadmium, magnesium, or aluminium. Thenard states that he was unable to obtain the peroxides of magnesium or aluminium; and though he mentions having obtained a peroxide of zinc by means of peroxide of hydrogen, yet he regarded it apparently as too unstable to isolate. In spite of this assertion of

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* The printing of this paper was postponed for reasons stated in the following paper (see page 152 below).

Thenard's, it does not seem to have been generally believed that zinc did form any definite oxide higher than ZnO, if, in fact, it formed any other at all. The following quotation from Brandis seems to represent fairly the general idea on the subject held by chemists:—

"Thenard has described a peroxide of zinc obtained by agitating the hydrated oxide with oxygenated water; at all events, this is no permanent compound, and certainly forms no distinct salts with the acids. We may therefore reject the suboxide and the peroxide of zinc as indefinite compounds, and consider this metal as susceptible of one degree of oxidisement only, forming the protoxide."

Under these circumstances we thought the subject of sufficient interest to warrant us in bringing it before the Society, although we have not as yet obtained any of the peroxides mentioned in the title of this paper in a state of purity, nor are we as yet prepared to assign any formula to any one of them. Yet the results we have obtained prove the existence of peroxides of these metals, and tend to suggest the formula of the peroxides of zinc and cadmium.

The preparation of these peroxides in a state of purity, or even an approximation to purity, demands attention to minute details, comparatively slight variations making a very decided difference in the products obtained or even preventing their formation.

After a considerable number of experiments we obtained zinc and cadmium compounds containing a high percentage of oxygen; but we have not been so successful in the case of magnesium or aluminium, partly perhaps, because we have not had time at our disposal to try a sufficient number of experiments under varying circumstances.

The following are the results we have obtained:—

To a solution of zinc sufficient ammonia is added to redissolve the precipitated hydrate, and to prevent its reprecipitation on dilution with water to two or three times its former volume. If to this solution a dilute solution of peroxide of hydrogen be added, a copious white or slightly yellow precipitate is formed. This in itself points to some alteration having taken place, as if the precipitate were still hydrate of zinc, there is apparently no reason why it should not dissolve in the ammonia which is present. This precipitate, when filtered from the liquid and washed on the Bunsen pump six or seven times with cold water, possesses the properties of a peroxide. On adding a portion to a solution of iodide of potassium acidified with hydrochloric acid iodine is set free. After drying at a temperature of 100° C. it still retains its properties. It gives off oxygen on heating (from one sample of about $\frac{1}{4}$ of a gramme we collected 15 cc. of oxygen); on treatment with strong hot hydrochloric acid it gives off chlorine, it oxidises acid solutions of ferrous sulphate. These properties it retains almost undiminished after prolonged exposure to a temperature of 140° C.

In preparing the zinc compound we used a solution containing 5 grm. of zinc sulphate to the cc. To a 100 cc. of this solution 450 cc. of a solution of ammonia (0·037 NH_3 to the cc.) was added, and the whole filtered to remove the small quantity of precipitate left undissolved. To 100 cc. of the filtered solution 350 cc. of a dilute solution of peroxide of hydrogen was added, which produced a copious precipitate. This was collected on a filter, and washed rapidly with cold water on the Bunsen pump, and then dried on a water bath. The substance thus obtained is a yellowish-white powder, which on ignition yields protoxide of zinc, water and oxygen being evolved. It dissolves readily in cold dilute acids, and this solution gives, on shaking it up with chromic acid solution and ether, the blue coloration characteristic of peroxide of hydrogen.

In order to estimate the peroxide oxygen, we dissolved weighed portions in standard solution of ferrous sulphates acidified with sulphuric acid, and then titrated with standard solution of permanganate of potash. The percentages of oxygen estimated in this way, in three different portions, were 6·92, 6·92, and 6·42 respectively. On ignition the substance lost 20·9 per cent.

The composition, and notably the percentage of oxygen, vary very considerably when the method of preparation is slightly modified. We prepared three portions, A, B, and C, in different ways.

A. 100 cc. of the ammoniacal solution of zinc were poured into 250 cc. H_2O_2 solution, 100 cc. H_2O_2 solution added, and the precipitate filtered, and washed with a very dilute solution of H_2O_2.

B. 350 cc. of H_2O_2 solution were poured into 100 cc. zinc solution and filtered, and the precipitate washed with very dilute H_2O_2 solution.

C. 350 cc. H_2O_2 solution were poured into 100 cc. of the same zinc solution, and the precipitate filtered and washed with cold water.

The three portions were all dried at 100°, and yielded on titrating the following percentages of oxygen :—

A,	6·27.
B,	7·2.
C,	5·6.

The analysis of Zn, B gave the following results :—

ZnO	= 77·1
H_2O	= 13
CO_2	= 4·1
O	= 7·2
	101·4

Deducting carbonate of zinc represented by 4·1 per cent. CO_2, and the water the percentage of O = 9·66.

On heating B to 140° for ten hours it lost 9·36 per cent., and the residue on titration gave 5 per cent. of oxygen. This shows a loss of oxygen as well as water.

In the preparation of the cadmium compound we used a solution of nitrate of cadmium containing 0·5 grm. to the cc. On the addition of ammonia and then of H_2O_2 solution, a precipitate is obtained which varies very considerably in colour according to the quantity of ammonia added. On adding 27 cc. of our ammonia solution to 100 cc. of the cadmium solution, a buff-coloured precipitate is produced by the H_2O_2 solution; but if a very little more or a very little less ammonia is added, the precipitate is much paler in colour, and approaches in appearance more nearly to the ordinary white hydrate of cadmium.

Three portions, A, B, and C, were prepared as follows :—

A. To 100 cc. of the cadmium solution 27 cc. of the ammoniacal solution were added, and then an equal bulk (127 cc.) of H_2O_2 solution, the precipitate filtered, and washed with cold water.

B. The same quantities were used, but the liquid was filtered from the precipitated cadmium hydrate, and H_2O_2 solution added to the filtrate.

C. Was obtained by washing the precipitated hydrate from B with H_2O_2 solution.

A B and C were then dried at 100° C., and on titration yielded the following percentages of O :—

A,	5·95 per cent.
B,	5·25 „
C,	4·93 „

The analysis of A gave the following results :—

CdO . . . . . = 85·8

H_2O . . . . . = 6·9

CO_2 . . . . . = 3·1

O . . . . . . = 5·95

101·75

The properties of this substance resemble closely those of the zinc compound. It is more decidedly yellow in colour, however, and readily yields on ignition the ordinary brown oxide of cadmium, water and oxygen being given off. On heating a portion of A to 140° for several hours, it was partially decomposed, and became browner in colour. This substance, on titration, gave 7·4 per cent. of oxygen, deducting carbonate of cadmium as represented by 3·1 per cent. CO_2 and the water the percentage of O = 7·34, in the anhydrous oxide.

For the preparation of the magnesium compound we used several solutions of sulphate of magnesium of different strengths, and adding varying quantities of the ammonia solution. The quality of the product varied in each case according to the mode of preparation.

Magnesium A was precipitated from a solution containing chloride of ammonium and free ammonia, and was filtered, after standing all night in contact with the liquid from which it was precipitated. The precipitate, dried at 100° C., on titration, was found to yield only 0·29 per cent. of O, showing that it was nearly all magnesia. The effect produced by varying the quantity of ammonia added, while all the other conditions were kept as nearly as posssible the same, is well shown in the following three oxygen estimations :—

Magnesium B contained 3·92 per cent. O.

„ C „ 3·1 „

„ D „ 2·63 „

Magnesium B was prepared from a solution to which ammonium was added till a very slight turbidity was produced, and then peroxide of hydrogen added.

In the case of magnesium C, the quantity of ammonia was somewhat increased, and in magnesium D to a still greater extent.

Other portions of the magnesium compound were prepared under slightly different conditions, and two portions were dried over sulphuric acid *in vacuo*, but without showing any decided difference.

The dry powder obtained from these precipitates, by drying them at 100° C., possessed properties similar to those of the zinc and cadmium compounds, viz., they set free iodine from an acidified solution of iodide of potassium, &c.

Magnesium B was selected for further analysis, as it contained the greatest percentage of available oxygen. On ignition it lost 36·15 per cent., and was found to contain 2·17 per cent. of CO_2, 30·9 per cent. of water, and to leave a residue of 60·85 per cent. of magnesia.

The percentage composition was therefore—

MgO	63·85
H_2O	30·9
CO_2	2·17
O (by titration) . .	3·92
	100·84

Deducting carbonate of magnesium represented by 2·17 per cent. CO_2 and the water the percentage of O in the anhydrous oxide = 6·03.

In the case of aluminium our methods had to be somewhat altered, owing to the insolubility of alumina in ammonia. We tried a solution of alumina in caustic soda, containing no more alkali than just sufficient to dissolve the alumina. 100 grs. of ammonia alum were dissolved in the smallest quantity of water, and caustic soda added until the hydrate at first precipitated was nearly all, but not quite, redissolved. When this solution was filtered and peroxide of hydrogen added, a copious white precipitate was obtained, which was filtered off, washed, and dried—one portion at 100° C., and another over sulphuric acid *in vacuo*. A third portion was allowed to stand in contact with some of the mother liquor till the next day, and then filtered and dried at 100° C. We called these respectively A, B, and C.

On titration they gave the following results :—

A, 1·04 per cent. of oxygen.
B, 1·6 „ „
C, 1 less than on percentage of O.

B was selected for further analysis. On ignition it lost 41·35 per cent. The percentage of water found was 36·05, and that of CO_2, 3·5. The percentage of alumina 58·65. The percentage composition was therefore

Al_2O_3	58·65
H_2O	36·05
CO_2	3·5
O (by titration)	1·6
	99·70

On deducting the water and CO_2 the percentage of oxygen equals 2·69 in the anhydrous oxide.

The presence of carbonic acid in this preparation is probably due to absorption from the atmosphere during the preparation. The water and carbonic acid were estimated directly; the oxygen by oxidising a standard solution of ferrous sulphate, and titrating the excess of ferrous salt by means of a standard permanganate of potash solution; the oxide by igniting a portion of the substance, and estimating the residue.

Although the above analyses show that the substances we have prepared are not pure, yet they hold out the hope that, by modifying our experiments, we may yet succeed in preparing the pure peroxides in a state fit for analysis and for the determination of their formulæ.

5. On Papers, by MM. Haas, Clève, and Lecoq de Boisbaudran, on the Production of Peroxides by means of Peroxide of Hydrogen. By J. Gibson, Ph.D., and R. M. Morrison, D.Sc.

The foregoing paper was read on 5th July 1880. We postponed, however, its publication, feeling that however interesting the formation of the bodies therein described might be, much careful quantitative research would have to be undertaken before any definite conclusion as to the true nature of these bodies could justifiably be drawn. Only the title of our paper was therefore published in the *Proceedings* of this Society.* The old University Laboratory being in many respects unfitted for the prosecution of accurate quantitative work, our researches were repeatedly postponed in the hope of being able to continue them in the new University Laboratory, which, however, only now approaches completion. This delay has been to us a subject of much regret, the questions involved being of no little interest.

We have still in our possession specimens of the various preparations now nearly five years old. When last examined, viz., during the Tercentenary Celebration of the University in April of last year, when they were shown to several of the distinguished visitors, they retained their properties to all appearance unchanged.

A week or two ago we observed a communication by M. Lecoq de Boisbaudran,† which commences as follows:—

"La récente publication d'un travail étendu de M. Clève relatif á l'action de l'eau oxygénée sur les terres rares (Yt_2O_3, Ce_2O_3 ThO_2 etc.) m'engage à ne pas poursuivre les essais que j'avais commencés dans la même voie. Je me bornerai à exposer ici quelques observations se rapportant à la peroxydation de Ce_2O_3 et de ThO_2."

M. Clève, in his valuable paper (published January 1885),‡ which deals mainly, though not exclusively, with peroxides of the rare earths, refers to a paper by R. Haas, § entitled "Ueber Peroxyde in der Zinc Magnesiumgruppe," dated 30th September 1884, which we had not previously noticed, and which covers very much the same ground as our paper, dated 5th July 1880. Under these circumstances, the verbatim publication of our paper is rendered necessary, in order to maintain our right to continue our researches. There can be no doubt that further investigation is very desirable.

In the first place, Messrs Haas and Clève state that they have not been able to obtain peroxides of aluminium and beryllium by means of peroxide of hydrogen. This we claim to have done in the case of aluminium, and some preliminary experiments lead us to the belief that beryllium also is capable of similar peroxidation. There seems little doubt, however, that in these cases the compounds are much more difficult to obtain.

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* Vol. x. page 706.

† *Comptes Rendus.*

‡ *Bul. Soc. Chim.*

§ *Ber. d. d. Chem. Ges.*

In the second place, the true character of these bodies is still uncertain. It is certain that the substances described by Messrs Haas and Clève, as well as those obtained by us, were not pure substances but mixtures. Haas states that he endeavoured to obtain the peroxides of zinc free from admixed hydrate, but that he never succeeded in producing the precipitation of peroxide when the formation of hydrate was prevented with sufficient care (sorgfältig genug). This statement is, however, too indefinite, and leaves one very much in the dark. It is easy to prepare ammoniacal solutions of zinc salts which remain perfectly clear when diluted with three or more times their bulk of water, but in which the addition of their own volume of hydrogen peroxide solution produces an abundant precipitate. This behaviour makes it difficult to understand how the formation of the zinc peroxide is necessarily a secondary product depending upon a previous formation of hydrated oxide. Our experiments lead us to a quite contrary conclusion, viz., that the hydrated oxide is a secondary product, the result of the decomposition of the peroxide. The mere determination of the peroxide oxygen and of basic oxide does not give a satisfactory insight into the composition of these bodies unless the absence of acids (carbonic, nitric, &c.) is proved. How far this was the case with the substances obtained by Herr Haas cannot be gathered from his paper. Our preparations, as well as those of M. Clève, contained one or other of these acids. We expect shortly to be in a position to communicate the results of the continuation of our researches to the Society.

6. On Supersaturation. By W. J. Nicol, Esq.

PRIVATE BUSINESS.

The following Candidates were balloted for and declared duly elected Fellows of the Society:—Professor Steggall; James Pringle, Esq.; A. P. Laurie, Esq., B.Sc.; R. J. Harvey Gibson, Esq.; Professor Dyce Davidson; and George Smith, Esq.